

LINGUA

INTERNATIONAL REVIEW OF GENERAL LINGUISTICS
REVUE INTERNATIONALE DE LINGUISTIQUE GÉNÉRALE

Editors — Comité de Rédaction:

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VOLUME VIII, 1

January 1959

NORTH-HOLLAND PUBLISHING COMPANY
AMSTERDAM

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THE ORIGIN OF GRAMMATICAL GENDER I

Summary

From a general linguistic standpoint the origin of gender cannot be accounted for by universal causes that would apply to all existing and extinct languages and language families of the world. Future research should be concentrated on the concrete material and evidence of particular languages, and the result will have validity only in so far as the language family under scrutiny is concerned. The clue to the problem of the origin of gender, however, is to be looked for in the development of congruence. In any language the precondition of gender congruence is the formation of a limited number of substantival ending types. Natural gender, value-differentiation (*Wertunterscheidung*) and the habits and forms of thinking of a language community cannot bring about the category of gender, because the content of thought is not transferable to its external form, the system of language. Following Brugmann's and Meillet's basic views we can move nearer to the solution of the problem with regard to the Indo-European language family: gender appears here to be the result of the interplay of syntactic and morphological causes. The assonance-like agreement (**ek^hos dhabhros* – **ek^hā dhabhrā* – **ek^hōd dhabhrōd*, etc.) followed the morphological division of *genre animé* and *genre inanimé* which differentiated the nominative from the accusative, while the analogical force of the nouns ending in *-a* brought into existence the third paradigmatic system of the motion.

1

The category of grammatical gender (*genus*) is one of the still unsolved puzzles of linguistic science. It is not only with regard to the circumstances of its origin that there has never been a complete agreement between linguists, there still is much divergence of opinion as to its function in a particular language-system and, in general, as to its definition. Most of the issues to be elucidated, however, are linked up with the question of its origin. This is the starting point from which the solution of the other related problems might be approached ¹).

¹) The problem of gender has been treated from the point of view of general linguistics by all those who had some share in establishing that particular discipline: Bopp, Grimm, Paul, Wundt, Brugmann, Meillet, Jespersen, Vendryes etc. Several monographs on the subject have appeared from still others. Van Royen, G. reviews and, in part, criticizes the opinions and pronouncements advanced

If one wishes to put forward one's own opinions on this tangle of related questions, the concept of gender has to be, at the very outset, marked off from problems of another nature. Some scholars (W. Schmidt, Bubrich, Henning, Velten, Hjelmslev etc.) include among the phenomena of gender the case of the lexical difference between the forms of a pronoun referring to the difference of natural gender (sex) (male-female), and animateness-inanimateness (who-what) respectively. According to this view gender is differentiated in the third person singular of modern English personal pronouns (*he, she, it*), and in the forms of the interrogative and relative pronouns in the Finno-Ugric languages (Hungarian *ki-mi*, Finnish *kuka-mikä*).

The essence of gender, on the other hand, is more correctly stated by the opinion which makes the criterion of this category the principle of congruence. Thus, in any synchronic system the category of gender exists only, if a member of a sentence-element with variable gender and of a polyform paradigm system (which may be adjective, pronoun, numeral, verbal, verb) is regularly made to agree with a sentence-element (usually noun) of a single paradigm system and with invariable gender. That property of a variable gender word class, viz. that it has more than one form, is called motion: Germ. *guter Vater* – *gute Mutter* – *gutes Kind*; Russian *chorošij otec* – *chorošaja mat'* – *chorošeye diť'a*. Genus therefore is a grammatical category that manifests itself in morphological divisions brought about on the members of syntagmatic units standing in identical relation to each other (chiefly Subject – Predicate or Attribute – Qualified Word), but this will not cause any semantic difference. Genus is a syntactic phenomenon exhibiting morphological features ²⁾.

According to our definition the semantic differentiation based on

from the Greek philosophers down to 1929 in his work: *Die nominalen Klassifikations-Systeme in den Sprachen der Erde. Historisch-kritische Studie mit besonderer Berücksichtigung des Indogermanischen*, Mödling-Wien, 1929.

²⁾ Steinthal, H. (*Gesammelte kleine Schriften. I. Sprachwissenschaftliche Abhandlungen und Recensionen*, Berlin, 1880, pp. 384–390) was among the first to pronounce in favour of the view that the essential point in gender was to be found in congruence, but by basing himself on data collected from the Indo-European languages, he admitted agreement exclusively in the sphere of the attribute – qualified word relation, while considering the congruence of subject – predicate as a superfluous, or, worse, cumbersome drawback.

lexical differences of single words (heteronymy: Hung. *ki* – *mi* 'who – what', *színész* – *színésznő* 'actor – actress') does not belong to the phenomena of grammatical gender proper. The above definition, however, holds good only from a synchronic point of view. Historically, as we shall see later, gender distinction may arise from several causes, possibly due to lexico-grammatical interaction.

2

We can get nearer to the problem of the rise of gender by glancing at the history of the problem. I propose to deal with the most important theories only, because they cover the others any way. For this reason the order in this survey will be determined not by mere chronology but by the inner connexion between the members of a group ³⁾.

Greek and Latin philosophies of grammar were divided into two opposing camps in their estimation of gender. One view held that gender was only a meaningless form, devoid of content, the other maintained that it was a reflection of sex. It is rather characteristic that from the beginnings of modern linguistic studies the same difference of these two opinions is found to prevail in the theories concerning the origin of the category.

In the heroic age of grammar-writing for the modern European languages it was with some measure of pride that the gender distinction was discovered, or rather that the same three genders could be pointed out in the languages under scrutiny as in the much-honoured classical languages. Permeated by the idea of anthropomorphism it was thought reasonable and consequently superb that natural gender was reflected in language through such subtle grammatical devices and further, that through the neuter gender even those concepts could be set apart that were indifferent in point of sex. Following in the footsteps of Adelung and Herder, even Bopp ⁴⁾ and Grimm ⁵⁾ looked upon gender as the reflection of sex. Nevertheless,

³⁾ Van Royen op. cit. treats the history of the question systematically and in detail, adopting for the main part a chronological order.

⁴⁾ Bopp, F.: *Vergleichende Grammatik des Sanskrit, Zend, Griechischen, Lateinischen, Litthauischen, Gothischen und Deutschen*, Berlin, I. 1883. (p. 135).

⁵⁾ Grimm, J.: *Deutsche Grammatik*, Göttingen, III. 1831. (p. 311 ff).

some scholars, especially Grimm pointed out that the initial state of things had been radically changed in the course of linguistic history. Later on, the grammatical writers increasingly stressed how that "logical order" had become chaotic. They perceived that, as little as the masculine gender could always be employed to denote the male, or the feminine gender always indicate the female, could either of the two genders on occasion become the linguistic form of a concept belonging to the opposite sex. Thus, *der Hase* could denote not only the male of the hare, but the female as well, *die Maus*, similarly, does not mean primarily the female of the mouse, but the male just as well. This phenomenon is the so-called epicene gender (ἐπίκοινων).

La sentinelle means in French 'sentry, sentinel' who is usually a man, and yet the noun expressing this concept is feminine. What seems even more illogical: *das Weib* (meaning woman in German) is neuter, and so, similarly, is *das Frauenzimmer*. A host of other examples could be adduced from all the languages possessing the category of gender.

Furthermore, it was also noted that besides those words whose meaning had something to do with natural gender, thousands of other words, indifferent in this respect, belonged quite unaccountably to the one or the other gender. The lack of any logical system is even more striking if one considers that there are no two languages which would sort their stock of words into anything like identical gender divisions. One language has the word in the feminine gender which is masculine or neuter in another, and vice versa. Thus 'moon' is masculine in German (*der Mond*), whereas it is feminine in French (*la lune*).

But what is the most striking in respect of the correlation between natural and grammatical gender is the fact that the grammatical contrast between feminine and masculine is not always exploited, as one might expect, for the expression of female or male concepts, but, instead, the language resorts to other devices. As a rule, the problem is solved by lexical means, that is, by heteronymy: Germ. *der Hengst* 'stallion' – *die Stute* 'mare'; Russ. *byk* 'bull' – *korova* 'cow'.

In this connexion there exist some further anomalies. The noun *médecin* in French is masculine. It can mean male as well as female doctor. If, however, it is absolutely necessary to indicate that the doctor is "she", then French solves this exigency rather clumsily by the compound: *femme-médecin*. For as soon as *médecin* is changed into a feminine word by grammatical means (*médecine*), it will no longer

mean female doctor but 'medical science, medicine'. There are cases where such a course would not entail the difficulties caused by semantic change, and yet it is inadmissible. For instance, the female of the word *éléphant* could be *éléphante*, as this would be in accordance with the morphological rules of the French language, and yet there exists no such word in the literary language, the female elephant being called *éléphant femelle*.

The discrepancy between genus and sexus is likewise evinced by the phenomenon called common gender (*genus commune*). This means that a noun denoting person may be masculine or feminine alike, depending on whether it refers to man or woman, e.g. Latin *agricola, advena*, Greek *ἄνθρωπος, τιμωρός*; Old Church Slavonic *gostъ*; Russ. *brod'aga, raster'acha* ⁶⁾.

As is to be seen the languages which distinguish gender frequently leave this possibility unexploited and solve the expression of sex by the very lexical means as the languages innocent of gender, e.g. Hung. *férfi - nő* 'man-woman', *bika - tehén* 'bull-cow', *orvos - orvosnő* 'he-doctor - she-doctor', *hímelefánt - nőstényelefánt* 'male elephant - female elephant'. As Brugmann ⁷⁾ ably argues, it is precisely this circumstance which proves that gender is not the reflection of sexus and could not have originated thus that primitive man transferred into his language the thing which he took note of in his environment and his everyday experience, namely the sexual difference of living beings. If it had been so, then he would have employed the expression of sexual difference consistently where it could have been most suitably used: in the naming of human beings and animals of different sex.

All these contradictory formations, seemingly arbitrary and incompatible with logical order, apart from the logicians' criticisms directed against language from the side of philosophy, induced a few scholars to take up a view directly opposite to the prevailing one. Some thought that the category of gender was by no means something excellent of

⁶⁾ On the correlation and discrepancy of genus and sexus see Vendryes, J.: *Le langage*, Paris, 1950. (pp. 108-110). With special regard to Greek and Latin see Wackernagel, J.: *Vorlesungen über Syntax mit besonderer Berücksichtigung von Griechisch, Lateinisch und Deutsch*, Basel, II. 1926. (pp. 9-53).

⁷⁾ Brugmann, K.: *Das Nominalgeschlecht in den indogermanischen Sprachen*, *Internationale Zeitschrift für allgemeine Sprachwissenschaft*, 4, 1889.

which the Germans, the French, the Italians etc. could be justly proud, but, on the contrary, it was something disadvantageous and cumbersome, complicated but empty, an obstacle to simple expression. So von der Gabelentz ⁸⁾ writes: „Das grammatische Geschlecht bringt es weiter mit sich, dass wir Deutschen nie eine Frauenperson als einen Menschen und nicht leicht einen Mann als eine Person bezeichnen“. Jespersen ⁹⁾ mentions several similar views and gives some very characteristic examples. For instance 'person' in French (*personne*) is feminine, but, when it refers to a man, masculine concord is required: *les personnes que vous attendiez sont tous logés ici*. Or consider the clumsiness of this sentence in German: *Was Maria und Fritz so zueinander zog, war, dass jeder von ihnen am anderen sah, wie er unglücklich war*.

3

As is to be seen the phenomena of gender when put to critical scrutiny have shown multifarious, complex, and in many respects different features from other grammatical categories. The problem of the origin of this particular category has presented ever knottier problems.

The relevant theories can be divided into two main groups from the point of view of their content. To the first group, which is also the older one, belong the opinions about the reflection of natural gender. Its chief exponents were Bopp, Grimm, Wundt, W. Schmidt, Paul, Henning, de la Grasserie, G. H. Müller, Specht, Hirt, Vendryes etc. The second group includes those theories that reject the idea of sex as a starting point, and are trying to look for a clue in the inner laws of language. This standpoint is championed by Brugmann, Meillet, Lohmann, Güntert, Velten, Westermann, Martinet etc.

The views belonging to these two groups are not always sharply defined. Not unfrequently the exponent of one or the other view is not able to maintain his standpoint consistently. The theory of natural gender continued to exert its influence, and even those who

⁸⁾ Von der Gabelentz, G.: Die Sprachwissenschaft, Leipzig, 1901. (p. 235).

⁹⁾ Jespersen, O.: Language, its nature, development, origin, London-New-York, 1923. (p. 347). Cp. also Budenz, J.: A grammatikai genus hiánya, Nyelv-tudományi Közlemények, 16, 1881. Cp. note 79 to be published in Part II.

wanted to break away from it often found it hard to rid themselves of it altogether, and continued to make certain concessions to it. Of a mixed nature are the opinions of Meillet, Güntert, Lohmann, Paul, Henning, G. H. Müller, Specht, Hirt.

As far as the range of application of these theories is concerned we again find two big groups. To the first, which is also the earlier one, belong those opinions that claim general linguistic validity, extending the solution to all the languages or language families of the world, even when their evidence is supplied by one language family or only a few. Into this group fall the theories of Wundt, W. Schmidt, Paul, Henning, G. H. Müller, de la Grasserie, Güntert, Velten, Meinhof etc. To the second group may be relegated those scholars who wish to solve the problem within a given language family only. They do not, as a rule, claim for their views universal applicability to all languages, but their work has some such merit, partly for the significance of their theories, partly for the instructive character of their methodological procedure. The most important among them are: Brugmann, Meillet, Specht, Hirt, Westermann.

Among the theories mentioned there are many based on the assumption that the pronoun gave rise to grammatical gender, although they endeavour to trace back the origin in a different way, and supported by different motives. In the last analysis all these theories go back to the thoughts of Bopp who wanted to derive the Indo-European suffixes generally from the agglutination of the pronoun, first of all the demonstrative pronoun. The chief representatives of the views mentioned are Steinthal, Paul, G. H. Müller, Specht, Hirt, Martinet.

For my part, as I hope to expound later, I reject the idea of the reflection of natural gender, and join those who repudiate it. At the same time I disbelieve that the problem can be solved in an identical way for all the language families of the world.

Now I propose to pass in review the more important theories. Let us first deal with the criticism of the sex theory. In this view human thinking created a linguistic category for the sexual difference between living beings. The neuter gender was brought in to cover those

concepts that had nothing to do with sex. The disorder in this respect, touched upon above, was explained away by the linguistic development disrupting the supposed initial order through a series of sound changes and the force of analogy.

This view was first based on the material of the Indo-European languages. Moving beyond this family of languages, the linguists were faced with ever greater complexities. In the Semito-Hamitic languages the distinction between the masculine and feminine genders appeared to have been made on the same lines as in the Indo-European languages, but the threefold pattern of masculine, feminine and neuter genders could in no way be forced upon the phenomena of the Caucasian languages. A similar difficulty had to be faced in regard to the Dravidian languages, in some of which the three genders could be pointed out, but these showed considerable divergence from the Indo-European pattern. In the same way, those American Indian languages which developed the category for the distinction of animate-inanimate gender presented a peculiar picture. These showed no affinity to either Indo-European or any other family of languages. Lastly, the largest number of problems was caused by the Australian and African, chiefly Bantu, languages. In the Australian languages the number of gender is between two and nine – with regard to the Bantu languages it differs, some authorities distinguishing no less than twenty-one genders.

A few scholars, as for instance Meinhof ¹⁰⁾ and Dešerijev ¹¹⁾, think it doubtful whether so divergent phenomena showing so few common features, can at all be bracketed under the heading of the category of gender. In languages possessing many more than three genders, at present we still speak not of gender but of “nominal classes” (Nominalklassen). Nonetheless general linguistics was soon to discover and, in face of the sceptics, it still maintains today, that the common feature of the phenomena in question, that is, the formal congruence of one sentence element with another, constitutes the essence of these phenomena and thus, the nominal classes belong to the category of gender.

¹⁰⁾ Meinhof, C.: Die Entstehung flektierender Sprachen, Berlin, 1936.

¹¹⁾ Dešerijev, J. D.: Specifika projavlenija abstragirujuščej roli grammatiki v sisteme grammatičeskich klassov, Doklady i soobščeniya Instituta Jazykoznanija, Moskva, VII. 1953.

Passing beyond the evidence of the Indo-European languages it became clear beyond a doubt that the old views about the origin of gender could be maintained neither in connexion with the Indo-European language family, nor in general respects. For the origin of the category could not be explained by the theory of the reflection of sex, since in many languages the grammatical distinction of sex is either not discoverable, or it appears, overshadowed by the system of several nominal classes. Rather characteristically, the numerous nominal classes of the Bantu languages make use of this gender-like category for the expression of natural gender even less than the Indo-European languages do and, instead, employ compounds or heteronymy. Thus in Lamba the name of the cock is *kombolwe*, that of the hen *inseke*, in Koongo a young man (*di*)*toko*, a young woman *nduumba*, in Sotho king is *mofuma*, queen is *mofuma-hali* etc.¹²).

After the original conceptions applied to the Indo-European language family had broken down in the course of the investigation, in a broader field, of the gender category, a new solution had to be sought. It was Wundt¹³) who undertook the task. He broadened the concept of natural gender so as to be able to include the phenomena hitherto excluded from it. He tried to classify genders and nominal classes on the basis of the meanings of the nouns belonging there. The classes set up in this way correspond in Wundt's opinion with the manner of the classification used by primitive society to arrange the phenomena (living beings, objects, natural phenomena etc.) known to it according to their value, taking for basis some principle of magnitude or importance. This value-differentiation (*Wertunterscheidung*) was, in Wundt's view, the basis for the classification of meaningful words. Seen in this light, gender was nothing else than a reflection of value-differentiation. Wundt enumerates the following types of value-differentiation:

1. the differentiation of concepts of superior and inferior value; the first category to include the deities and other supernatural beings, while the latter one comprizes women, children, animals and objects and their names; a typical instance of this differentiation is exemplified by Iroquois;

¹²) See Meillet, A. – Cohen, M.: *Les langues du monde*, Paris, 1952. (p. 867).

¹³) Wundt, W.: *Völkerpsychologie*, I. Die Sprache, 2. Leipzig, 1900. (pp. 19–24).

2. the differentiation of human beings (including women) from all other concepts; Fulbe is a case in point, although there, children belong to the latter group;

3. the differentiation between animate and inanimate; instanced by American Dakota, though found also in some North Caucasian languages mixed with groupings of a different sort;

4. the differentiation of man and woman, which is extended to all other concepts; an example of this are the Semitic and Hamitic languages;

5. three-gender system; a classical example is furnished by Proto-Indo-European and most of the Indo-European daughter-languages.

The author adds that the distinction for three genera came into existence as a further development out of the two-gender system, when the concepts indifferent in point of sex could be placed with neither of the genders. But the distinction for sex is also based on value-differentiation, for between sex classes, a similar inferior-superior relationship was imagined.

Wundt also noted that the value-differentiation was only a starting-point in the rise of gender, the rest being done by the inner forces of language (analogy, sound change). All this, however, is only touched upon and not expounded in any detail by Wundt.

It is seen clearly from his whole train of thought that the theory of value-differentiation is, in fact, a development of the views concerning the reflection of natural gender. Wundt's explanation is to be considered an improvement on the previous theories, because he did not stop at the mystification of the role of sexus, but endeavoured to look for deeper, psychological and sociological motives.

Wundt's theory of gender earned him widespread acknowledgement, in spite of the rather severe criticism given to his *Völkerpsychologie*. The idea of value-differentiation made possible the survival of all hypotheses advanced since then on the sex theory. Wundt, therefore, can be said to have saved the natural gender theory whose adherents have not disappeared to this day. In illustration let me quote from Vendryes¹⁴): „Le genre, dans nos langues européennes, n'est qu'une

¹⁴) op. cit. p. 114. Among recently published general linguistic works similar thoughts are found, for instance in Drexel, A.: *Ursprung und Wesen der Sprache*, Zürich, II. 1952. (pp. 186-187).

classe" à la façon du bantou. Il représente une tentative faite par l'esprit pour classer les notions si variées qui s'expriment au moyen des noms. Le principe de ce classement répond sans doute à la conception que nos lointains ancêtres se faisaient du monde; des motifs mystiques et religieux ont contribué à le fixer. La tradition s'en est maintenue même après qu'on a cessé d'en comprendre la raison d'être".

Wundt's theory exerted its influence not only in its original shape, but also stimulated further development. W. Schmidt modified in some measure the basic types of the value-differentiation as conceived by Wundt, but effected essential alteration rather by his efforts to link the psychological features with certain stages of social development. On a basis not very dissimilar to that of Marr, he links the types of value-differentiation with those of primitive society (Kultur Kreise), that is, with the matriarchal, patriarchal, totemistic, nomadic animal-breeding, etc., stages of society. In his view, the form of society necessarily involves a corresponding type of value-differentiation as, indeed, the corresponding forms of any other grammatical categories ¹⁵⁾.

The theory of sex and that of value-differentiation seems evident and matter-of-fact enough, but it cannot answer one very important question, thereby revealing its indefensibility, to wit: what made it necessary for primitive man to clothe his perceptions about the world into grammatical form, or, to put it in another way, why did the conceptual classification of value-differentiation assume the form of gender-distinctions? Surely primitive man was able to manifest his attitude in everyday life according to his valuation of the things around him, even if he was unable to express the fact by grammatical devices in his speech. Many primitive peoples do employ value-differentiation in their way of thought without ever having developed the category of gender. To them belong the Finno-Ugric peoples, who lived once in

¹⁵⁾ Schmidt, W.: Die Sprachfamilien und Sprachenkreise der Erde, Heidelberg, 1926. (pp. 334-357). Schmidt's theory is not new, it is only the most elaborate and far-reaching in its influence. The ethnographers and the linguists that dealt intensively with ethnographical problems had hinted at the inter-connexion of gender and social development, or, in general between language and its other categories, and the development of society. See the discussion on Meillet's lecture: *Le genre féminin dans les langues indo-européennes* in his *Linguistique historique et linguistique générale*, Paris, II. 1952. (p. 25 ff).

nomadic tribes, and the Obi-Ugric people have only lately begun to emerge from that backward stage of society. Apart from this, there are a considerable number of languages which, as far as we can ascertain it today, never knew the category of gender and still lack it, although having passed through the very same stages of development as the Indo-European or any other peoples possessing in their languages grammatical gender.

Nevertheless the theory of value-differentiation meant a certain advance on the theory of the reflection of sex, although, as is revealed in the above test-question, it fares no better. It is an improvement in so far as it broadens the horizon beyond the Indo-European and Semito-Hamitic language families, although at the same time this advantage becomes a disadvantage. It is so general and abstract that it is unable to cope with the concrete facts of a given language, it cannot take into consideration the lessons taught by language history, it contents itself with examining the semantic peculiarities of gender, that is, it restricts the problem to the nouns only, it cannot direct its attention to morphological, syntactical phenomena. The problem of the origin of gender cannot be solved before the historical data of all the language-groups concerned are adequately looked into. For there is the possibility that the origin of genus cannot be accounted for by universally identical causes in all the languages of the world, for, with regard to its origin, gender is a heterogeneous phenomenon, various causes contributing to its appearance in the different language families and only at some later stage did it become a more or less homogeneous phenomenon. If this possibility exists, then any mode of investigation that attempts to clear up the problem of origin claiming applicability to human speech in general and basing itself on a few, chiefly descriptive data, can only lead into a blind-alley.

It is equally characteristic of both the value-differentiation and the theory of natural gender that they are not concerned with the provenance of gender. They fail to explain how the reflection came into existence, in what manner, by what grammatical means, through what stages of development.

The conclusion to be drawn from all this is that we have to give up the generalizing and vague explanations based on psychological and sociological motives, of the value-differentiation and natural gender theories, and the solution of the problem has to be sought by de-

tending more closely on the linguistic data and history of individual languages or language families. It was not easily that linguistics has come to recognize this truth.

It was Paul ¹⁶⁾ who ventured a step farther on the old road. Although he depends in his inferences predominantly on Indo-European evidence, he jumps at general conclusions. He takes as his starting point the sex theory and calls the neuter gender the gender of concepts which are indifferent to sexus. Superficially, his theory seems to be less valuable in comparison with that of Wundt, for he makes his generalizations on the basis of Indo-European material only. Its progressive character is shown, however, in that it holds congruence a very essential point in the rise of gender, and thinks that without concord, this category would never have grown out of a sheer desire to distinguish natural gender. Differing from all views held up to that time, Paul, therefore, looks for the starting point in the origin of gender not in the noun, but in the other variable gender word classes, more especially in the pronoun. The masculine and feminine forms of the pronoun served in his view as the basis of analogy for the motion of the other variable gender word classes. Unfortunately Paul does not follow up this supposed process in its entire evolution, he barely hints at some possibilities. One of these would be that the variable pronoun got stuck to the root of the noun in the manner of a suffix and the adjective agreed with that ending by the force of analogy. This possibility is not considered as certain by the author, who thinks it even likely that the frequency of agreement with certain roots came about by chance and only later did it give rise to the motion of masculines and feminines.

Paul goes on to speculate on the possible ways the gender distinction fitted into the language-system, and what factors played part in deciding the gender of a word. In addition to morphological and phonetic factors, and analogy, Paul points in this connexion to the role of man's imagination in personifying, sexualizing, as it were, his concepts with the help of gender distinction.

Paul's theory is extremely important because setting out as he does on already traversed paths he recognizes as the cause of the rise of gender the working of grammatical forces. On closer examination of

¹⁶⁾ Paul, H.: *Prinzipien der Sprachgeschichte*, Halle, 1909. (pp. 263-269).

Paul's thoughts one finds that there is little or no organic connexion between his assertions concerning the role of sex and his exposition of the agreement of variable gender words. Without doubt it is the latter which has stood the test of time, only it is regrettable that Paul hardly gathers together enough convincing proofs to back up his opinions, so far as the Indo-European languages are concerned. They are even less serviceable in the sphere of general linguistics. Paul's views on the role of pronouns later reappear in Hirt's theory ¹⁷).

Henning ¹⁸) set himself the task to develop further Paul's ideas about the role of the pronouns. He states unambiguously that the pronoun represents gender more decidedly, and this is seen more unmistakably in morphology than in the case of the noun. Beside the Indo-European languages he gives consideration chiefly to African (Hamito-Semitic, Sudanese, Bantu) tongues. He finds in many cases only the pronoun representing gender, while with other word classes the distinction is not traceable. This is to be seen especially in the South Hamitic languages, but elsewhere in the world too, in Singhalese and in several American Indian languages. According to Henning the distinction of natural gender (male-female) appears first in the pronoun, and it spreads to other word classes by way of grammatical means. This is shown by the fact, that the gender-indicating morphemes of the noun in Indo-European and in most of the languages he examined, are pronominal in their origin (?).

Apart from some hasty and unwarranted historical conclusions the flaw in Henning's theory shows up when he considers the heteronymy of pronouns expressing sex as gender distinction in a given language. In Singhalese, by the way, it is the pronoun that represents most clearly the category in comparison with the other word classes, but the gender distinction in that language goes back to Indo-European triplicity of gender, and thus it can furnish no basis whatever in

¹⁷) It should be noted here that the importance of the pronoun in the genesis of gender had been recognized by many before Paul came forward with his theory. Apart from the fact that since Bopp several scholars have tried to derive suffixes from pronouns, Steinthal (op. cit. p. 390) is of the opinion that the endings which were vital from the point of view of gender also came into being by way of the agglutination of pronouns (*bon-us de-us*).

¹⁸) Henning, R.: Über die Entwicklung des grammatischen Geschlechts, Zeitschrift für vergleichende Sprachforschung, 33, 1895.

deciding the issue of origin. It was doomed to failure from the beginning, for starting out from some particular concrete linguistic data to conclusions of general validity can be arrived at through such leaps of logic ¹⁹⁾.

I am going to treat of one more theory born of a commixture of different views. G. H. Müller ²⁰⁾ starting from Winkler's theory tries to reach conclusions on the scale of general linguistics. He considers gender distinction not as a reflection of sexus, but of the distinction between abstract and concrete concepts. He is at pains to discover this distinction in the Indo-European language family. The author investigates the meaning of cases by morphological analysis. He finds the distinction between the nominative and the accusative important from the point of view of gender, but in different context than, later, Meillet did. He calls the *-s*, *-sa* nominative endings of masculines the case of perception (*Kasus der Wahrnehmung*), and the neuter *-m* reflection: the case of image (*Kasus der Vorstellung*). He sees, therefore, the difference between cases or case endings not so much as functional, but rather as a semantic-psychological difference. He goes on to treat of the *-m* ending characteristic of the neuter gender as a particular case of abstraction as against the concrete masculine *-s* reflection, then he states that the meaning of feminine nouns was originally abstract (feminine quality), and in this respect these nouns were identical in meaning with the neuters. Thus, originally, the neuter and feminine genders stood in opposition to the masculine. The *-m* ending is derived by the author from the pronoun **ma*, which is taken to be identical with a homonymous verb root meaning 'to think, to desire' ('denken, begehren'). The abstract meaning was isolated in this way and became the feature of cases.

Unfortunately, very little remains of these conclusions that can be accepted. The author starts out from a fiction that is no better than a disguised variety of the natural gender theory. This he tries hard to

¹⁹⁾ Many scholars were to fall into this latter kind of error, notably Velten and Meinhof, both of whom will be discussed later. The development of gender in Sinhalese was on more complicated lines than described by Henning. Concerning this see Hjelmslev, L.: *Animé et inanimé, personnel et impersonnel, travaux de l'Institut de Linguistique*, Paris, I., 1956, pp. 175, 180.

²⁰⁾ Müller, G. H.: *Das Genus der Indogermanen und seine ursprüngliche Bedeutung*, *Indogermanische Forschungen*, 8, 1898.

validate with doubtful etymologies and other speculations. He makes no mention of congruence, instead, he grapples with semantic explanations, which are obscure and unacceptable.

In the following I want to deal briefly with two theories which, although relying exclusively on Indo-European material, have proved the most fruitful so far in helping towards a solution of the problem. Neither of them went further, however, than their evidence permitted. I have in mind the opinions of Brugmann and Meillet.

Brugmann ²¹⁾ takes a definite stand against the natural gender theory, looking for an explanation of the origin of gender in the inner laws of language, above all, in the force of analogy. In his study he tries to shed light on why the *-ā-* and the *-ī-*, *-iē-* declension stems turned feminine and not into some other gender. In his opinion what happened was nothing else than the abstraction of the stem suffix by means of irradiation. This kind of phenomenon is not uncommon in the history of Indo-European languages. In illustration he mentions the case of the suffix *-yo-* meaning colour, and the suffix *-bho-* denoting animal. The *-yo-* suffix had originally nothing to do with names of colours. But in one word, the adjective **ghel-yo-s* 'yellow', the suffix had been abstracted on account of the force of the stem, and it became in Latin and Germanic languages a colour suffix: Latin *fulvus*, *flavus*; Old High German *salo* 'black, dirty' etc.

The original function of the suffix *-bho-* also had similarly no connexion with names of animals, cp. Old Indic *sthūlabhas* 'rough, massy'. Later, in the Indic, Greek, Celtic languages, it was abstracted as a suffix of animals: Indic *rāsabhas* 'ass', Greek ἔριφος, ἔλαφος etc. In a similar way had the *-a* suffix in the *-ā-* declension been abstracted due to the characteristically feminine meaning of the nouns **māmā* 'mother, mamma' and **gʷenā* 'wife' and came to be established as a female suffix. Analogous with that had come into being **ekʷā*, a variant pair of the word **ekʷos*, in the meaning of 'mare'. The process of abstraction was, in all likelihood, rather a long one, since a number of important male-female semantic opposites had been expressed by way of heteronymy in primitive Indo-European. The *-ī* ending in the *-ī-*, *-iē-* declension came to be abstracted as a female suffix on account of the meaning of **strī* 'woman'. After the

²¹⁾ op. cit.

names of female beings developed separate suffixes, the congruence of the adjective became established by analogy.

Brugmann was aware that certain difficulties were to be overcome to get his theory accepted. Therefore, he seeks to explain why some of the nouns belonging to the *-ī-* declension became feminines. In accounting for that phenomenon he supposes further associations of meaning. His arguments are as follows: the *-ā-* noun stem subsumed many abstract words (Latin *fuga*, *juventa*, Old Church Slavonic *льѡа* etc.); there are quite a few of these abstract nouns in the *-ī-* declension, e.g. **mētis* (Latin *mens*); it is likely, therefore, that the adjective agreeing in form with the abstract nouns in the *-ā-* stem, preserved its ending alongside the word **mētis* by way of external analogy, because of the similarly abstract meaning of that noun, and hence the *-a* ending of variable gender words spread by way of internal analogy throughout the whole declension type. In addition to this hypothesis Brugmann does not think it impossible that adjectives had originally possessed variant forms, only, these did not agree with the noun endings, but each had a separate meaning. Thus the *-a* ending of adjectives is not seldom used in the Indo-European languages in abstract sense, e.g. the feminine pair of the masculine form of the Lithuanian *geltas* 'yellow' means also 'jaundice'. The feminine of Latin *noxius* masc.: *noxia* is commonly used as a noun for 'sin'.

Brugmann's theory has that outstanding merit that it tries to find the solution in the inner workings of language mechanism, instead of in extrinsic motives. To the other positive points of the paper I shall have occasion to return later; here I am concerned with pointing out its weak sides. Among these the least objectionable seems to me that which met with Jespersen's strictures ²²⁾, namely the view that only a couple of words with characteristically feminine meaning furnished the starting-point for irradiation. Yet these nouns, precisely because of their meaning, must have occurred very frequently in the spoken language. The very examples mentioned by Brugmann show that sometimes it was exactly due to the meaning of a single noun that the ending acquired such an additional meaning and function.

The graver fault in Brugmann's hypothesis lies in his stopping at the emergence of the feminine gender. He has a few words concerning

²²⁾ op. cit. pp. 391-394.

the -o- declension nouns, but he never so much as mentions the rise of the neuter gender. Perhaps, carrying his reasoning further, the explanation in the latter case, too, would have to be found in some external semantic analogy. The author does not, however, continue his investigation. Thus the way in which the separation of the other two genders took place is wrapt in mystery. Was the masculine gender isolated in the next stage, and, left-over, did the neuter gender become crystallized, or the other way round? Did the separation of all three genders take place perhaps simultaneously by way of the three basic analogies? To all these queries we get no answer from Brugmann.

The author's theory concerning the original motion of the adjective is hardly acceptable, although, it must be added, he only hints at this possibility. For one thing, it is rather unlikely that all three variant forms of the adjective were ever in use as nouns with different meanings. No such instance can be pointed out today. In any case, they could have played no important part, as abstract quality could be expressed not only by the feminine form of the adjectives, but by their masculine and neuter forms alike in substantival use, e.g. Old Indic *jānas* 'creature' masc. – *jānā* 'birth' fem., *gharmās* 'warmth' masc., *yajnas* 'honour' masc.; Greek *τόμος* 'cutting' masc.; Latin *dubius* – *dubium* 'doubt' neuter. Lithuanian feminine examples are of no documentary value as, there, after the extinction of the neuter gender, either the masculine or the feminine gender could have taken over the function of expressing abstract quality, and, as the case of syntax proves, the feminine gender generally took the place of the neuter (*taĩ gražũ* 'this is beautiful', *mán šalta* 'it is cold [to me]'). Still it is not impossible that the substantivization of adjectival forms, as conceived by Brugmann, made for an easier development of gender concord.

Brugmann's explanation for agreement developed by external analogy seems hardly satisfactory. The historical evidence does not warrant much a supposition, it proves, on the contrary, that semantic analogies of this type played no essential part in the evolution of the Indo-European languages. Thus in modern Russian, the undeclinable place names of foreign origin may take occasionally, beside neuter concord, other congruence too, through the influence of external analogy. For instance, *Missisipi* may take feminine concord (*Missisipi tekla*) on the analogy of the feminine gender of the generic word *reka* 'river'. So may for similar reasons the name of the town Timbuktoo (*Timbuktu*

prosnuls'a) be masculine on the analogy of masculine *gorod* 'city, town'. But the number of such cases is negligible compared to the factors that decide the gender of a noun, let alone the fact that the meaning of the nouns providing the basis of analogy in the above examples is far more conspicuous than the common trait of **mptis* and *fuga*, that is, the abstract character.

Brugmann's hypothesis contains serious shortcomings, just because he chose a wrong starting-point. What he asserts about the formation of the female suffixes is quite acceptable, but it does not bring us any nearer to the solution of the problem. For had the motion of variable word classes not come about, then those suffixes would in themselves have been insufficient to bring about the category of gender, since in genderless languages, like the Finno-Ugrian, the Turko-Tartaric language families, Japanese, and Yoruba mentioned by Brugmann, we come across female suffixes, some of which may have indeed been formed by way of irradiation, and yet the category of gender did not become established in those languages. The clue to the problem lies hidden not so much in the suffixes or the noun, but in the development of the motion of variable gender words.

In this respect Brugmann has practically nothing to say, and what little he does say is not always acceptable.

Meillet ²³⁾ approaches the problem from a different angle. Instead of operating with morphological and semantic motives, he looks for syntactical relations. He thinks the separation of the three genders in Proto-Indo-European did not come about simultaneously. First the non-neuter (animate gender – *genre animé*) became detached from the neuter gender (inanimate gender – *genre inanimé*). This separation was caused by the need in certain syntactic situations of distinguishing the subject from the object, which was, initially, not possible because of the identity of the nominative and accusative. In certain syntactic situations the identity of form in the two cases caused ambiguity, as in this hypothetical Common Slav sentence: *synъ viditъ otъsъ*. Originally it was impossible to distinguish the subject from the object, the agent from the patient because of the

²³⁾ Meillet, A.: *Linguistique historique et linguistique générale*, Paris, I. 1948. (ed. pr. 1921). pp. 199–229, II. 1952. (ed. pr. 1926). pp. 24–28, and his paper: *Essai de chronologie des langues indo-européennes*, *Bulletin de la Société de Linguistique de Paris*, 33/1, 1931.

free word order. This difficulty was eliminated by providing the accusative with a separate suffix but only in those cases when this was felt absolutely necessary. Persons, living beings or personified concepts stood for the agent, the other things were generally patients. Man hits, cuts the tree, but the tree does not hit, cut man. For this reason the names of objects figured less as nominatives, and thus in the case of nouns denoting things the nominative and accusative did not require formal distinction. Hence, the nominative of these nouns stood for both the nom. and the acc., unlike the nouns of the other group. Out of this former group developed the neuter gender. Indeed, Proto-Indo-European is characterized by the formal identity of the nom. and acc. of neuter words to such an extent that the several thousand years of development could bring forth only a few exceptions in this respect.

The next stage was, in Meillet's view, a splitting within the animate gender itself, as the influence of natural gender brought about the separation of the masculine and feminine genders. Quite unaccountably Meillet in his three previous papers does not detail this process beyond simply declaring it. He points out, however, as an important circumstance, that without the congruence of the adjective the separation could not have taken place. One can agree with this last statement with the qualification that it applies equally to the separation of the *genre animé* and the *genre inanimé*, considering the fact that the identity of the nom. and acc., or their difference of form, is found even in those languages where there is no congruence (nor has the category of gender developed). In the Finno-Ugrian languages besides the formally distinguished object relation, there can be found presumably older unmarked forms, which are divided in their respective use according to certain syntactic conditions, yet without having brought forth the category of gender ²⁴).

Meillet characterizes the differences in the successive emergence and development of the three genders by stating that in the declension of adjectives the neuter gender is set apart from the other two genders by the identity of the nominative and the accusative, while the feminine gender is distinguished from the masculine first and foremost

²⁴) As for the evidence concerning the Finno-Ugrian languages, chiefly Hungarian, see Klemm, A.: *Magyar történeti mondattan*, Budapest, 1940. (pp. 271-283).

by the difference of word formation. From the point of view of gender, the difference between the forms *bonus* and *bona* lies in the difference of the stems *bono-* and *bona-*. Most of the feminine forms originated in masculine forms. Thus out of the masculine *-ont-/-nt-* of the present participle came the *-ntī/-ntjā* form.

The French linguist in a later, his fourth, paper (Essai) finds it necessary to deal in more detail with the separation of the masculine and feminine genders. He does not relinquish his former views and essentially traces back the separation of the genre animé to natural gender, but he stresses now the importance of congruence in bringing about the bifurcation. Here too, he says, it was not the adjective, but again the motion of the pronoun, namely that of the **so/sa* pronoun (Sanskrit *sá*, Greek *ὁ*, Gothic *sa* masc. nom. – Sanskr. *sā*, Greek **hā* (see Ion-attic *ἡ*, Gothic *so* fem.) that played the decisive part; this later spread on to the **to/ta* demonstrative pronoun, which displays root difference only in the nominative and accusative, hence, by way of analogy of the abstract adjectives (one, whole, the same) to the *-o-* and *-ā-* stem adjectives and then, finally, it embraced the nouns. **Ĝ^{enā}* was of less importance in the sphere of nouns, nevertheless it might also assist to bring about motion. With nouns denoting ‘woman’ or ‘female occupations’ a more influential factor was the emergence of the suffixes that emphasized the agency of the female (Lat. *-trix*, *genitor* masc. – *genetrix* fem., **-n-yā* Sanskr. *pātnī*, Greek *πότνια* etc.). Meillet does not go further in this correction of his than Brugmann, as he only puts forward other, less satisfactory possibilities for the analogy that led to motion. Even if he is aware of the fact that concord crowned the process that led to the separation of the animate gender, yet he cannot make his proposition acceptable that the two forms of the *so/sa* pronoun, which he considers as of pioneering importance, had to conform of necessity to the female agent suffixes. At any rate, the fact alone that the pronoun has two paradigm systems, presupposes its motion. Meillet thus wants us to accept here a case of *filius ante patrem*. While he is able to describe convincingly the important syntactic peculiarities of the agent and the patient that were conducive to the separation of the animate and inanimate genders, his theory about the role of the nouns denoting female beings in splitting in two the animate gender becomes as nebulous a fiction as the whole natural gender theory.

On the other hand, Meillet's view seems to be very much acceptable when he asserts – this is by the way his main thesis in the essay – that the differentiation of the genre animé was the outcome of a long process, and it possibly took place during the period when the prehistoric Indo-European languages were splitting up into separate dialects. This would account for the less differentiated character of the feminine and masculine genders in some of the daughter languages.

The great merit of Meillet lies in the fact that, like Brugmann, he sets about solving the problem through a fresh approach. Especially noteworthy is his view that the three gender forms came into existence not simultaneously, and, further more, that dissimilar causes must have played part in giving rise to them ²⁵).

The gist of Meillet's theory can already be discovered before him. Quite a few of the adherents to the natural gender theory are for putting the separation of the genders at successive periods, some of them arguing for the priority of the neuter gender, others for the animate (lebendig) gender, that is, the masculine and feminine genders, on the basis of various speculations ²⁶).

²⁵) In his previous essays Meillet puts forward doubtlessly interesting comparisons which are, however, inconclusive proofs as regards the origin of gender. He is at pains to prove that the concepts of water and fire developed in the Indo-European languages names of different gender according to whether they were conceived as active or passive (Sanskrit. *udakām* neut. – *āpah* fem.; Sanskrit. *agnih* fem. – Greek *πῦρ* neut.), moreover, the names of certain heavenly bodies and parts of the body, etc. assumed whatever gender they now have just because of their imagined active or passive properties.

²⁶) De la Grasserie, R.: (De la catégorie du genre, Paris, 1906. I. pp. 235–236, 246–247) also standing on the ground of the natural gender theory, is of the opinion that in the Proto-Indo-European language the neuter gender emerged last as the weakest gender. Its weakness is shown by the circumstance that in the Indo-European languages it could persist the least, being first to disappear. The order of the other two genders, on the other hand, was 1. masculine, 2. feminine. De la Grasserie can not in the least validate this last assertion of his. He provides no proofs whatever to justify that order.

Müller, F. (Grundriss der Sprachwissenschaft, I. 1. Einleitung in die Sprachwissenschaft, Wien, 1876. pp. 122–123) considers the division of two genders as original in the world's languages in general. This state was preserved in its purity in the Semito-Hamitic language family. There the difference of the subject and the object, that is that of the spontaneous (agent) and the recipient (patient) became established. The Indo-European triplicity of gender was the result of further divisions. There, the subject was divided into the absolute

Meillet's ideas regarding the separation of the animate and inanimate genders have gone unchallenged to this day and so the supposition of the later bifurcation of the animate gender seems to be well-founded ²⁷).

The greatest shortcoming of Meillet's theory is the fact that he concurs with the fiction of the reflection of sex in accounting for the separation of the masculine and feminine genders. This great thinker, therefore, could not go beyond the teachings of Bopp and Grimm in this question.

It has been attempted by numerous other scholars to develop further Meillet's ideas ²⁸). Lohmann ²⁹) marshalling a wealth of evidence, sets himself the task of establishing the relations between the masculine and feminine forms in point of word-formation on the one hand, and the way of the previous development of the neuter on the other. Unfortunately he has nothing new to say about the division in two of the animate gender, which the author terms polarization.

Vaillant ³⁰) tries in his interesting article to back up Meillet's

active (das Spontane als unbedingt Tätiges) masculine, and the feminine attendant on the action (das Tätige Begleitendes). Müller fails to go into details, and his arguments of a syntactic kind are very obscure. The least substantial point is his generalization for all the world's languages.

²⁷) Specht, F. (Der Ursprung der indogermanischen Deklination, Göttingen, 1944. pp. 300-301) does not accept the idea of the division of genre animé and inanimé. Substantially however, it is acknowledged by the whole train of his thought. Specht holds the original function of heteroklisis to be the distinction of the direct case and the oblique case (pp. 350, 337-338, 391, etc.). This distinction was later replaced by a more advanced means when this function was taken over by the difference of the -s and -m suffixes, deriving from pronominal elements, thereby forcing heteroklisis into the background.

²⁸) Güntert, H. (Grundfragen der Sprachwissenschaft, Leipzig, 1925. pp. 54-59) unaware of Meillet's studies, supposes stages of development similar to those of Meillet's but, less convincingly, in reversed order. As an out-and-out adherent to the natural gender and value-differentiation theories, he gives priority to the separation of the masculine and feminine genders, while thinking the emergence of the neuter gender as secondary. In the latter assumption he is in essential agreement with Meillet in sketching forth the situation.

²⁹) Lohmann, J.: Genus und Sexus, eine morphologische Studie zum Ursprung der indogermanischen nominalen Genusunterscheidung, Ergänzungshäfte 10. für Zeitschrift für vergleichende Sprachforschung, Göttingen, 1932.

³⁰) Vaillant, A.: Sur l'évolution du genre des cas et des parties du discours, Bulletin de la Société de Linguistique de Paris, 37, 1936.

opinions from another angle, taking to a certain extent, a different road. The separation into animate-inanimate gender is traced back by him to the difference of an original ergative case (later nominative ending in -s) and the nominative (-s -less nominative), where the subject construction in the ergative helped the rise of the animate gender, while the subject construction in the nominative helped that of the inanimate gender. Unlike Meillet, he derives the subsequent appearance of the feminine gender from the inanimate gender, namely that the *-ā-* declension ending (*-a*), which was at the same time the neuter collective suffix, spread. As to how this came about Vaillant's description is cursory and difficult to follow. The important thing is that he seeks to bring syntactic factors to bear on his explanation. However, the analogy he cites is not clear: "Le genre féminin est né de la syntaxe d'accord, et de l'opposition de 'l'homme est bon' (sujet et prédicat animés) et 'la femme est chose bonne', 'l'ἀνᾶρθεος est chose bonne' (sujet animé, prédicat inanimé)".

Martinet ³¹⁾ takes up the line where it was left off by Meillet, that is, the separation of the masculine and feminine genders. In his view the vital part was played by the pronoun, namely the adjustment of the demonstrative pronoun ending (**se/so*), in the separation of the feminine gender. Martinet admits that it is not the formation of female suffixes, but the grammatical agreement that constitutes the essential point of gender. According to the author, beside the word meaning woman, thus chiefly **ǵ^uenā* and perhaps some other similar words, the demonstrative pronoun so as to better show its concord with those words, assumed their endings. This new *-ā* ending spread over to the other pronouns that displayed motion by way of analogy, then to the adjectives, later to all words – not only those denoting females and ending in *-ā*, and finally it spread to the accusative and the other oblique cases.

Martinet sees correctly that the essential point of gender is congruence, but he fails to explain why only the demonstrative pronoun took on the ending of **ǵ^uenā* and for what reasons. Was it because thereby it could better show its relation to that word, that is, to the concept denoted by the word ("...on désire, dans l'emploi pro-

³¹⁾ Martinet, A.: Le genre féminin en indo-européen: examen fonctionnel du problème, Bulletin de la Société de Linguistique de Paris, 52/1, 1957.

nominal, marquer la référence aux notions désignées par ces mots'')? Why only the demonstrative pronoun and not also the other words used attributively, and why was it necessary only in connection with words denoting female beings? And if it was necessary to employ this distinctive ending with words denoting female beings, why did the demonstrative not acquire a **sī* form alongside **strī*? The author leaves these queries unanswered, and his train of thought is inconsistent because he makes concessions to conceptual and semantic factors. Otherwise Martinet's opinions show points of contact with those of Brugmann, although he does not refer to him.

One more essay must also be mentioned here, which shows points of contact with Meillet's thoughts, but arrives at different conclusions on a quite different road. I am referring to Velten's paper ³²⁾.

Velten makes general statements extending his findings in the Indo-European languages to all the world's languages. The author takes the view that out of all grammatical categories only one can be found in all the languages: the category of person. It was called into being by a syntactic necessity to distinguish the subject from the object in such a manner that it was laid down in the form of the third person of the pronoun. Later on the personal pronouns became person endings and these, in turn, gave rise to gender divisions.

Velten regards personal inflections not as verbal endings, agreeing with the opinion that the distinction of the verb and the nominal word classes is of later origin, but as the distinctive inflections of the subject and the object respectively.

The gender distinction is therefore originally based on that of animateness and inanimateness. Apart from the rather obscure re-formulation of the concept of person, we find Meillet's separation of the animate and inanimate genders reappearing in Velten's work, but turned into a generalization extending beyond the Indo-European data. While, Meillet builds on concrete evidence, Velten bases his propositions on just a few examples culled from here and there.

From then on Velten proceeds entirely on his own. He is of the opinion that the categories of gender and number are interlinked, the one without the other is inconceivable. In illustration he mentions,

³²⁾ Velten, H. V.: Sur l'évolution du genre des cas et des parties du discours, Bulletin de la Société de Linguistique de Paris, 99, 1932.

among others, Chinook, in which exist masculine, feminine, neuter genders, singular, plural and dual numbers, but a masculine noun can only be masculine or plural, a feminine noun can only be feminine or dual, etc. He considers that the establishment of many genders is due to the complication of gender and number. Besides, he claims that the category of gender necessarily comes into existence in all the world's languages for the very reason of the category of person being existent in all languages. The distinction in the relative pronouns of the Finno-Ugric languages (Hungarian *ki - mi*), and the four hypothetical plural endings in the primitive Finno-Ugric language can be considered to be the remnants of a previous complete gender system ³³).

There can be found very little that is helpful in Velten's argumentation. To assert the universal existence of the person category is an exaggeration, even if the concept of person is completely reformulated. The connexion of gender and number seems to be more real indeed, but more caution in this respect would have been welcome. There are languages which developed number, while showing no trace of gender (Velten thinks it simply disappeared), e.g. in the Finno-Ugric and Turko-Tartaric language families. The evolution of the two categories can be quite independent of each other, but it is an undeniable fact that they are at times interlinked. There is much investigation to be done in this sphere, not so much in general linguistics but rather in the particular languages ³⁴). I shall later return to the necessity or non-necessity of the establishment of gender, remarking here that Velten's evidence in connexion with the four Finno-Ugric plural inflections is at fault because these cannot be traced back to primitive Finno-Ugric. None of the Finno-Ugric languages possess all four, and so they are rather the results of later separate development in those languages ³⁵).

An entirely different road is taken by Specht. He has some scattered

³³) It should be noted that the "heteronymy" of the Finno-Ugric interrogative and relative pronouns are regarded by Bubrich, D. V. (*Istoričeskaja morfologija finnskogo jazyka*, Moskva-Leningrad, 1955. pp. 45-46) as the remnant of an initial nominal and pronominal class distinction.

³⁴) Hjelmslev (op. cit. p. 197) regards the categories of gender and number as being of the same origin.

³⁵) Concerning this see Ravila, P.: *Über die Verwendung der Numeruszeichen in den uralischen Sprachen*, *Finnisch-Ugrische Forschungen*, 27/1-3, 1941.

remarks in his outstanding study on the origin of Indo-European gender. He does not devote a separate chapter to this problem, but he has a few words on it in various places ³⁶⁾, in connexion with other problems. His view can be patched together like a mosaic, and a mixture emerges of the natural gender theory and Brugmann's ideas. The author thinks he can discover in the differences of Indo-European stems the distinction of animateness and inanimateness. He is of the opinion that the heteroclitic words forming part of the oldest layer of the Indo-European vocabulary came into being through animation, personification, primitive man transferring his imagination into the declension stems with the help of pronouns. If we throw overboard the mythological superstructure we get the Boppian thought on the role of the pronoun which at least tried to explain how the concepts of sex were translated into language. Specht presumes that the gender distinction is independent of the formation of stems, being a secondary phenomenon. The process came about in a way that the suffixes *-nī*, *-nīa* etc., containing an *n* pronominal element, were attached to words denoting other than male beings, and it was in this way that they received feminine meaning. The original feminine meaning was not carried by the ending *-a* but by the *n* element. The contrast that these formative suffixes were found only in words designating female beings as against the lack of these formative elements in words denoting male beings, gave rise to the separation of the feminine and masculine genders (e.g. Old Indic *pātnī*, Greek *πότνια*, Old Lithuanian *wiešpatni*, Old Church Slavonic *bogynī*). The establishment of the neuter, in the author's view, took place simultaneously with the other two genders, in such a way that any word which did not belong to either of the groups, remained separate, expressed formally by the root, i.e. it remained unmarked. Specht does not go further and has less to say of variable gender words than Brugmann, attributing to them only a secondary role in the establishment of gender ³⁷⁾.

There is a crying discrepancy in Specht's views between the declarative part and the documentation. What little concrete material he commands in his investigation of the morphological factors in the establishment of gender, is, in fact, far from being in favour of sex,

³⁶⁾ op. cit. pp. 301–302, 306–307, 385–386, 391 etc.

³⁷⁾ op. cit. pp. 112, 117.

but on the contrary against it, as it was with Brugmann. Apart from our objections raised earlier to similar theories, here I may allude to the fact that the contradistinctive function of the suffixes *-nī*, *-nīa* etc. could have provided a starting point only if the neuter had not come into existence. On the other hand, Specht's not quite new opinion that the Indo-European declension stems evolved independently of gender has to be accepted. Incidentally, Specht makes no reference to Brugmann's views.

I have to deal with one more important theory concerning the Indo-European language family. It is Hirt's theory ³⁸). He knows well the works of Brugmann and Meillet (except of course Meillet's later *Essai*), but does not approve of them. He does not go out of his way to a thoroughgoing refutation of them, he is only concerned with his own ideas, in which he gives prominence to the pronoun. Hirt's view shows points of contact with Meillet's work in so far as he too separates the neuter gender from the other two, as of different origin, and further, he similarly believes the masculine and feminine genders to be the reflection of natural gender.

The author differs essentially from Meillet in the question of the evolution of the neuter. He too holds it to be older compared to the masculine and feminine, but not because its nominative and accusative coincided, but because he presumes that it is a remnant of an earlier, genderless state of the language. His arguments clearly show he is in total disagreement with Meillet in this respect. According to him the neuter is indefinite case (*casus indefinitus*) (*ius*, *mare*, *ἕδωρ*) from a morphological point of view, the *-om* suffix being a later development of pronominal origin. Hirt also takes into account the testimony of syntax, but in a different way than Meillet did. The kind of congruence occurring when a neuter adjective or pronoun refers to a neuter noun is considered by him to be a remnant precisely of the genderless state (*omnium rerum mors est extremum*. — *ὅν ἀγαθὸν πολυκοιρανίη*) and not as a noun feature of the adjective or the pronoun.

The core of Hirt's theory resides in his thought on the establishment of the masculine and feminine genders. He decidedly states that the gender division is not linked with the declension of nouns, for declension had originally nothing to do with gender: nouns of different

³⁸) Hirt, H.: *Indogermanische Grammatik*, Heidelberg, III. 1927. pp. 320–347.

gender belonged to a particular stem. On the other hand, the pronoun, more particularly the demonstrative pronoun, and the original formal difference of the third person personal pronoun respectively, brought about the motion of the other pronouns and the adjective, which, helped by congruence, resulted, in the last analysis, in the category of gender. The difference of form in these pronouns (Greek δ , η , Iranian *sa*, *sā*, Latin *is*, *ea*, Old Indic *aj-ám*, *ij-ám* etc.) was characterized by the fact that the feminine gender was denoted by the $\bar{a}$ and $\bar{i}$ element. But why? We get no answer from Hirt, he only makes the objection to Brugmann that the *sā* or $\bar{a}$ pronoun, respectively pronominal element, was far likelier to have feminine meaning ('she') than the $*mā$ or the $*g^h enā$ on account of their ending. In the final evolution of gender and in the fact of individual nouns belonging under whatever gender, Hirt allows a great role to animation, sexualization.

I take one more rather important part out of the context of Hirt's disquisitions. He admits adjectival motion as being the result of later development, however, the adjectival $-\bar{i}$, younger than $-\bar{a}$, may have come from the pronoun $*s\bar{i}$ or possibly a pronoun $*\bar{i}$, both with the meaning of 'she' ³⁹). In the pronoun like words the masculine was denoted by the *s* sound while the neuter by $\bar{d}$.

Unfortunately Hirt could not make a step further than Brugmann and Meillet, he even fell short of them. The unauthenticated analogy of an original 'she' meaning of the $*s\bar{a}$, $s\bar{i}$ is less acceptable than Brugmann's suggestion. But even if we were to accept these pronouns as originally referring to females, Hirt can in no way prove why the *s* had a masculine meaning in the form of pronouns, as this element is not thought by the author himself to have come from an independent word, but as being a demonstrative particle ⁴⁰). The problem of the establishment of the masculine is left unsolved not only by Brugmann who totally neglects it, but much more so by Hirt, who deduces the origin of gender not from mechanical agreements, but from the division of the pronoun into decidedly masculine and feminine meaning, and attributes a far greater role to sex and sexualization than Brug-

³⁹) Hirt op. cit. pp. 111, 333 and 330. Cp. also Brugmann, K. (Grundriss der vergleichenden Grammatik der indogermanischen Sprachen, Strassburg, II. 2. 1903) pp. 86-87. Others believe the $-\bar{i}$ - stem to be of later origin. See Gray, L.H.: On Indo-European noun-declension, especially of $-o$ - and $-\bar{a}$ - stems, Language, 8, 1932.

⁴⁰) Hirt op. cit. p. 140.

mann. Hirt's theory also falls short of Meillet's in that his syntactic arguments are weaker than those of his predecessor.

5

Before proceeding further I have to deal with the part the pronoun played in the origin of gender. As has been seen, a long line of scholars from Bopp to Hirt, raise very much the same thoughts on the subject however far their arguments may differ.

First it must be examined whether the pronoun of independent use has any gender.

In connexion with the languages possessing gender category where the pronouns do not change their form in all persons in the sense nouns and variable gender words do, or always remain invariable, one cannot speak of gender proper. But undeniably the pronoun has, to a certain extent, something to do with gender even in this case. Let us take this example, Russian *ja znal* 'I (man) knew' – *ja znala* 'I (woman) knew'. The gender of the predicate varies according to the pronoun referring to a man or a woman. The basis of agreement in the present case is the meaning of the invariable pronoun. Let this case be termed *semantic gender* in contradistinction to the agreement of variable gender words, which may be called *grammatical gender*.

In that case, i.e. when the pronoun displays motion, the situation changes. Let us take the following example, Russian *on znal* 'he knew' – *ona znala* 'she knew' – *ono znalo* 'it knew'. The predicate is adjusted to the pronominal subject, but the pronoun itself alters its form according to the gender of the person or thing it stands for. In this way the pronoun is only an intermediary in point of gender and it behaves differently from both the invariable and variable gender word classes: it agrees while it is also being agreed with. Let us call this case *grammatical-semantic gender*.

Surveying the behaviour of the pronoun in different contexts it would appear at first sight as if the relation of genus and sexus were much more immediate. The idea may even arise that the coming together of these two different phenomena were, in fact, effected by the pronoun. But this relation shows only on the surface. The semantic concord of the pronoun could never have come about without the analogy of the pronoun having motion, at most simultaneously, but

never previous to it. As for the type *on znal* – *ona znala* – *ono znalo* it must be taken into consideration that the agreement can refer not only to man and woman, but to any masculine, feminine or neuter noun (*on byl* = *dom byl*, *ona byla* = *zima byla*, *ono bylo* = *solnce bylo*). It is of some consequence too that the pronoun of this kind became abstracted in the majority of languages (thus for example in Indo-European) from the demonstrative pronoun or else from its attributive function (Old Church Slavonic *онъ* or *тъ* *čelověkъ jestъ* > *онъ* or *тъ* *jestъ*). The agreement of the demonstrative pronoun functioning as attributive element is an instance of the grammatical gender.

According to Hirt only the personal pronoun or the demonstrative pronoun in that function could have started the gender division into separate forms, i.e. motion. The Indo-European third person pronoun came into being considerably later than the category of gender, consequently the motion of the pronoun could not come about due to its use as personal pronoun, but only to its attributive and predicative employment.

Proceeding beyond the sphere of Indo-European languages and investigating the gender-forming possibility of the pronoun in other language groups, the conviction is strengthened that we are on the wrong track. In the genderless languages, it is generally not felt necessary for the pronoun to have one separate form referring to males and one referring to females. The question arises, just why should it have been so in Proto-Indo-European? It is hardly probable that the pronouns **sī*, *sā* had originally the meaning 'she', as Hirt would have us believe. That could only take place after, and by no means before, congruence became established.

In passing I add a remark that in Japanese there are two forms, one form for man and one for woman, of the third person of personal pronouns, without the language having gender category. The following few pronouns have more forms: *ano otoko* 'he' – *ano onna* 'she', in plural *ano otokotachi* 'they (male)' – *ano onnadomo*, *ano onnatachi* 'they (female)'; there is even a "neuter" pronoun: *ano mono* 'it' etc. But these pronouns, as most of the Japanese personal pronouns are substantival in origin (*onna* 'woman', *otoko* 'man', *mono* 'thing'), so that we are faced with only a commoner and more general use than in Hungarian where a person or thing expressed by a pronoun is more closely specified: *az a férfi* 'that man', *az a nő* 'that woman', *az a*

dolog 'that thing'. Yet this can not be called gender division since there is absolutely no motion involved. As long as the pronoun retains its pronominal nature and does not become a suffix, it can have no part in the formation of gender. As for primitive Indo-European no one as yet could convincingly prove the pronominal provenance of declensional suffixes. Thus Bopp's idea has remained a mere hypothesis. In other language families, as it will be seen later, the pronouns did have a part in the evolution of gender and nominal classes respectively, having demonstrably become suffixes. In regard to the Indo-European languages, the problem of the establishment of gender can be approached without having need for that hypothesis.

6

After the sifting of all the theories concerning the Indo-European languages, only two conceptions remain that can be used: the views of Brugmann and Meillet. Let us now try to pick out the precious core of those two scholars' theories, and in this way get nearer to solving the problem.

We have to set out from the assumption that it was the genre animé and inanimé that drifted apart first, as it has been plausibly enough demonstrated by Meillet. Of course, it would be legitimate to object to him the counter-argument: why had language development by fixing word-order not eliminated ambiguities that might well have arisen? It is well-known that the Indo-European languages often choose precisely that makeshift, after sound changes had made the nominative and accusative alike in form, cp. French *le père voit le fils* – *le fils voit le père*. To this it may be answered: fixity of word-order plays a major syntactic role only when fewer morphological resources are at hand. On the other hand, when the language abounds in affixes and its entire system is marked by a richness in relational elements, it is far more likely to employ the more accustomed and frequent method in responding to a new expressional need. Primitive Indo-European was characterized by just that richness of form as against the neo-Latin tongues.

According to the other possible counter-argument, the discrimination of agent and patient was superfluous as the number of cases must have been very few, when the formal identity of the nominative

and the accusative caused ambiguity. It is true that in some such hypothetical sentences: *otъcъ viditъ synъ*, it was really difficult to decide whether the father saw his son or the other way round, without the situational or speech context, but in the majority of cases, the meaning of the sentence supplies a sure enough guidance. In such a sentence like "the father instructs his son", the meaning of the verb shows unequivocally which part of the sentence is the agent and which the patient. It cannot be assumed then that a few instances of ambiguous context could have activated the inner laws of language to effect such new and sweeping changes affecting the whole structure of the language, so much less as ambiguity cannot be eliminated completely from any language. For instance in this Russian sentence *čelovek v beloј šl'ape pošel s nimi*, the adverbial *v beloј šl'ape* can belong as much to the subject as to the predicate, and the meaning of the sentence is altered accordingly in either case.

To this second counter-argument it must be answered that in the absence of texts, we can have no knowledge of primitive Indo-European sentences, consequently it cannot be demonstrated that ambiguous contexts were in preponderance, nor can the opposite be proved either. The counter-argument could be accepted only if it were able to authenticate its assertion. Meillet's argument, on the other hand, is supported by the evidence of linguistic history. In every case, when in the separate development of the daughter languages the form of the nom. and acc. coincided again, the agent and the patient were discriminated either by word-order or by morphological means. The neo-Latin languages chose the way of fixing the word-order. In the Slavonic languages, on the other hand, we are faced with a repetition, as it were, of the process that presumably had taken place in Proto-Indo-European, of course in a different setting. The phenomena just referred to commenced already in Common Slav and it is found in a developed state in Old Church Slavonic. In the *-o/-jo-*, *-ъ-*, *-ь-* and some consonantal stem declensions the nom. and the acc. coincided due to sound changes (*stolъ, synъ, gostъ, dьnъ*). In nouns denoting male beings the genitive began to be used syncretically in place of the accusative sharing the same form with the nominative, e.g. *izvolъkъ nožъ udari raba archiereova...* Cod. Mar.; *vojini že vьse Isusa vьnотrъ na dvorъ*. Cod. Mar.; *i obrěto Marijo i Josifa mladъnesъ*. Cod. Ass. Out of this developed the well-known category in the Slavonic languages

of the distinction between person and non-person, and between animateness and inanimateness respectively. The separate development of the Slavonic languages therefore made the discrimination of person, that is, the agent necessary; the same discrimination of the nom. and acc. can be seen as regards the masculine words (the overwhelming majority of words denoting females fell under the *-ā/-jā*-declension, where the nom. and acc. did not coincide); the variable gender words also followed this discrimination in their congruence (Russian *dobryj den'* – *dobrogo otca* acc.), with the restriction that this new accusative remained with names of persons and living beings, and did not form a new declension system, for this would have entailed difficulties in the completely developed Slavonic inflectional system. It is rather interesting to notice that similarly to Slavonic, Spanish among the neo-Latin tongues did not confine itself to fixing word-order, but it started to express the acc. of living beings by the dative: *yo veo el mar* – *yo veo al padre, a la madre, al ave* ⁴¹). I shall return to this problem later.

After this digression, let us trace further the evolution of gender. At the separation of the two accusative forms, coincidentally with it, the variable gender words might have begun to submit to concord. Nothing is known of the details of this process so important from the point of view of gender. As has been seen, this problem is only touched upon, but has never been analyzed by linguists even if they speak at length about its importance. Brugmann himself only briefly outlines it, and besides he is on a wrong track.

In my opinion the establishment of motion has its cause in the analogical influence of the ending of the invariable gender noun. The sentence elements placed in close relation with it, first and foremost, the parts of speech employed attributively, agreed with the termination of the noun in a rime-like assonance to show their belonging: **ek̑os dhabhros* – *ġ^{enā} dhabhrā* – *uergom dhabhrom* – *sūnus dhabhrus* – *matēr dhabhēr* sing. nom.; *ek̑ōd dhabhrōd* – *ġ^{enās} dhabhrās* sing. abl., etc.

Assonance-like motion of this kind, brought about by analogy, is

⁴¹) Hjelmslev (op. cit. pp. 175–176) considers this phenomenon as belonging to the category of person vs. non-person rather than to animateness vs. inanimateness, even if the accusative of nouns denoting animals is also identical with the dative, as is shown by our example (*yo veo al ave*).

not unknown in the later history of the Indo-European languages. Two examples of it can be seen in the inflection of the postpositive article, in Slavonic languages. In modern Bulgarian the article, especially in the plural, agrees not so much with the gender form as with the noun ending. In the singular the consonantal termination is no obstacle for the article to adapt itself to gender, although there is frequent wavering as to concord, e.g. *grad* masc. – *gradăt*; *smelost* fem. – *smelostta*, but *pepel* – *pepelăt* or *pepelta*. In the plural the article of nouns ending in *-a* is invariably *-ta*, independent of gender: *brat'ata* masc. – *knižata* (collective plural of *kniga*) fem. – *selata* neuter. Originally the article agreed with the inflected forms of the noun according to the appropriate declension pattern, but with the attrition of analytic inflection other forces began to operate, and it was in that way that the assonance-like congruence established itself, which in Bulgarian, diminished the expressive power of gender as far as the cases in question are concerned.

Similar phenomena are found in the northern dialects of Russian. Here the nominatives of both numbers show comparatively regular concord in their postpositive articles, but in the oblique cases only assonance is observable according to the noun endings. Historical data go to prove that here too, an earlier regular concord gave place to assonance ⁴²).

Even if in both cases we have to do with a development that weakened gender, it reveals that assonance has its sphere of operation in the life of the Indo-European languages.

The evolution of assonance should not be conceived of as some purely mechanical process; it had rather an important part to play: in consequence of the free word-order, it marked the relation between the members of syntagmatic units. The slowly evolving agreement did not at once mark gender distinction, for it only conformed to the word endings whose form was determined by the stems and the suffixes. Just as many case endings a noun had, in just so many forms did a member of a variable gender word class agree with it. Only after the two accusative terminations had come into existence, did the marking of gender become possible. The variable gender word class here too followed closely the appropriate accusative termination. At

⁴²) See Sobolevskij, A.I. (Lekciji po istoriji russkogo jazyka, Moskva, 1903). p. 224.

this point the adjective **dhabhros* developed two paradigms, as it got two acc. endings alongside nouns belonging to the same declension (e.g. *-o-* stem). In one, the nominative was identical with the accusative, in the other, the two cases were represented by different suffixes: 1. **dhabhrom* nom.-acc., 2. **dhabhros* nom. – **dhabhrom* acc. For the establishment of gender distinction, for the separation of the genre animé and genre inanimé it was necessary that the two phenomena should approximately coincide: firstly, the differentiation of the nominative and accusative and their identity respectively within the same paradigm system between the various groups of words belonging there, and secondly the establishment of concord. After the first step the development took the way as sketched out by Brugmann. The essential point here was not, however, that a new feminine suffix was isolated from the nouns **ġ^uenā* and **strī*, but the circumstance that variable gender words adjusted themselves to those endings. It is evident that the feminine gender became stabilized only gradually through the wearing off of the manifold terminations of variable gender words, and they became fewer and fewer until the variable gender words had only a few endings, and these few different endings referred to nouns with manifold endings. Thus through the influence of the concord **ġ^uenā dhabhrā*, **ek^uā dhabhrā* the **strī dhabhrā* agreement took the place of earlier **strī dhabhrī*. The meaning of **ġ^uenā*, **ek^ua*, **strī* denoting woman or female served for the basis of analogy here.

The only question now is why the manifold terminations of the variable gender words did not persist, and why precisely the *-ā* ending overcame the suffix *-ī* in the nominative. The reason was that the more frequently occurring nouns, more exactly their endings, ousted the less frequent ones by analogical extension. This was especially likely to take place when the same variable gender word was made to agree with more nouns of various endings. In the latter case it was evidently the more commonly used ending that set the pattern: **sā ġ^uenā ʁe strī*, **ġ^uenā ʁe strī dhabhrā* etc. Furthermore the *-ā-* stem declension attracted a greater number of commonly used nouns than the *-ī-* pattern. But it was not only the definitely feminine meaning that was of consequence, but rather the termination itself, as many words markedly denoting male beings ended in *-a* (Latin *scriba*, Old Church Slavonic *vojevoda*). In the final establishment of

concord with the latter words some other analogies proved also effectual, although it may be that they led, straight from the beginning, to variant forms and wavering usage as to concord.

The outlined process is likely to have taken a similar course in all the other cases, too, if at different times. The variable gender word classes rallied in three paradigm systems well distinguished as to gender. They are characterized by the feminine gender form differing from the other two sets in both numbers and in all cases by distinct ending, while the masculine continued to be set off from the neuter by the nominative and the accusative suffixes respectively. With invariable gender nouns the surest distinction between masculine and neuter is that between nominative and the accusative, while with the feminine nouns the number of suffixes distinctly marking gender is smaller as compared to the variable gender word classes. So on account of the masculine words in the *-ā-* declension the suffixes of this type are hardly characteristic of the feminine gender exclusively. Compared to primitive Indo-European, the distinctive gender suffixes increased in the course of later development in those languages where the syntactical declension has been preserved.

The process sketched out above must have been accomplished amidst much wavering and uncertainty in putting concord into effect. A process of integration can be observed in the whole declensional system, which goes hand in hand with the elimination of less productive stems and their fusion into more productive ones. This phenomenon is also found in the declension of nouns, but it was more rapid and radical in the inflection of adjectives. While for example the modern Slavonic languages still more or less preserve the remnants of all the Indo-European noun declension patterns (Russian *-vrem'a* < *vreme*, -nt- sing. *telenok*, pl. *tel'ata* < *tele* - *teleta*, -r- *mat'*, dat. *materi* < *mati* - *materi* etc.), the declension of adjectives is represented only by the *-o-* and *-ā-* stems. In the Baltic languages, on the other hand, the conjecturally original motion of adjectives in the *-ū-* and *-ī-* stems has been preserved (Lithuanian *saldūs* 'sweet' masc. nom. - *saldī* fem. nom.; *didis* 'great' masc. nom. - *didė* fem. nom.).

It must be pointed out that the assonance-like endings were not assumed by all the adjectives in the beginning, and even they followed the oblique case endings. There remained only one termination in the nominative, thereby distinguishing the masculine (or the femi-

nine) and the neuter by the identical or different ending of the accusative (e.g. Latin part. praesens nom. masc., fem., ntr. *ferens* – acc. masc.; fem. *ferentem* – acc. ntr. *ferens*). The situation varies from one branch of languages to another. In the Slavonic languages the present participle became flexible (*bery* masc., neuter – **berotji* fem.). Generally the motion of *-es-*, *-ǐ-*, *-n-* adjective stems evolved in very few places. Several Indo-European languages preserved completely inflexible adjectives too, e.g. OCS *svobodъ*, Greek *ῥοδοδάκτυλος* etc. This phenomenon is explained by the fact that the unproductive stems contained little used adjectives. Because of their rare occurrence they did not develop assonance-like concord, or only to a limited extent.

Hirt ⁴³⁾ traces back the difference of adjectives of two endings (Latin *maior* masc., fem. – *maius* neuter) to the original distinction between animate and inanimate while explaining the participles of one ending (*ferens*, *audax*) by a later coincidence of form. However, he too thinks it possible that elsewhere the inflexible adjectives are archaic forms.

The motion of adjectives might have depended on other circumstances, chiefly their syntactic functions ⁴⁴⁾. Specht ⁴⁵⁾ advocates the view that the paradigm system of the *-ǐ-* stem adjectives is secondary.

Certain difficulties have to be removed before the course of development as mapped out above may qualify for acceptance. Is the possibility consistent with my arguments that the *-ī* feminine suffix is older than the *-ā* suffix? It has previously been hinted that several varieties and much wavering of concord must have prevailed until finally the *-ī* became crystallized, before the *-ā*, out of all the noun

⁴³⁾ op. cit. pp. 330–332.

⁴⁴⁾ Concerning this see the theory of Stang, Chr. R. (Zum indoeuropäischen Adjektivum, Norsk Tidsskrift for Sprogvidenskap, Oslo, 17, 1954). According to the author two kinds of adjectives can be supposed in Proto-Indo-European, namely the appositive (appositionell) and the descriptive (attributive and predicative). The appositive adjectives preserved their noun characteristics and did not develop motion. To this phenomenon can be traced back the fact that adjectives of one ending have no motion. If there may be raised an objection to this theory, viz. by pointing to the *nomina agentis* character of the appositive adjectives, doubtlessly the difference of syntactic function had sometimes strengthening, sometimes weakening effect on motion becoming widespread.

⁴⁵⁾ op. cit. pp. 112–113, and Zur Bildung der adjektivischen u-Stämme, Zeitschrift für vergleichende Sprachforschung, 65, 1938.

formatives. With the variable gender words, on the other hand, the *-ā* ending might have been the commoner.

Does it not run counter to our thesis, what Meillet hinted, viz. that certain feminine adjective forms were derived from the masculine? This question has not at all been satisfactorily cleared up. It is only the *-nt-* > *-ntjĩ* that might present a crux. This is also bound up with the circumstance of not every adjective declension developing motion. Evidently also morphological difficulties had played their part beside the relative rarity of words belonging to the unproductive stems. The fact is that the three different gender forms of the commonest declension pattern of adjectives could have come only from two noun declension types, that is, from the merging of the *-o-* and *-ā-* declensions into a single integrated threefold paradigm system. The less productive adjective declensions, in the event of developing motion, could not directly make use of such mixed solution, but if need was they availed themselves of word formation.

The initial uncertainties of agreement are best shown by the fact that there was no unity as to gender in a particular declension type in Proto-Indo-European. It was possible for a word with more than one gender to belong to one declension. A rather long development intervened before the stems had become more uniform in point of gender. This came about in two ways. The word either changed its gender, or altered its declension by going over to another type proper to its gender. The originally neuter *medъ* became masculine in Old Church Slavonic through the influence of the rest of the *-ŭ-* stem nouns; the **snūsos*, on the other hand, went over from the *-o-* declension to the *-ā-* stem in OCS in the form of *snъcha*, retaining its feminine gender.

There remain in this process of integration some minor questions of detail that call for elucidation. They include for instance the problem of quite a large number of masculine nouns belonging to the *-ā-* declension. Brugmann⁴⁶⁾ is of the opinion that they were feminine originally and had abstract meaning, but later being applied to men, turned masculine (Latin *auriga*, *scriba*, OCS *vladyka*, *vojevoda* etc.). According to the generally accepted opinion these words had always been masculine⁴⁷⁾.

⁴⁶⁾ Grundriss pp. 96–99.

⁴⁷⁾ See Meillet op. cit.

The question may arise whether the value-differentiation has any indirect role in the origin of the Indo-European gender in so far as the independent words originally might have stood for particular classes, then, losing their independence, became formatives and so set up declension stems similarly to the way, as will be seen later, nominal classes became established in Bantu. At the moment Indo-European linguistics cannot adequately settle this problem ⁴⁸⁾. This question will crop up again in connection with Meinhof's theory.

But even if the stems themselves originated in an earlier class or some similar distribution, gender could not have developed from that process, as this category came into existence definitely later than the formation of stems. This is shown most clearly by the fact that gender and the system of declensional stems are two quite different matters. In Proto-Indo-European, gender and stem did not constitute a unity, but both preserved their independence. It seems beyond argument that the stems and the variety of their terminations made easier the establishment of gender. The circumstance that stem and gender systems were two different things in Proto-Indo-European does not mean that they cannot be identical in some other language family.

In the separate development of the derivative Indo-European languages gender and stem came ever closer together through the process of integration already outlined. There are modern Indo-European languages where they became completely fused. For instance, in modern High German, where the noun declension can be divided into three stems according to the three genders: *der Vater* masc. – *die Mutter* fem. – *das Kind* neuter. Elsewhere this process could not run its full course. In the Slavonic languages, for instance, noun stem and gender stand apart even today, although the historical development evidences their gradual coming closer from Old Church Slavonic onwards. Complete fusion took place in Bulgarian and Macedonian, but, in consequence of the analytic system, it is hardly possible to speak of stems in these languages. In the other Slavonic languages non-

⁴⁸⁾ See Brugmann, K.: *Kurze vergleichende Grammatik der indogermanischen Sprachen*, Strassburg, II. 1903. p. 285. Hirt op. cit. pp. 81–84 believes in this hypothesis more firmly. Specht (Ursprung) explains precisely by this theory the establishment of Indo-European inflectional system and derives heteroclisis from the enclisis of the demonstrative pronoun.

conformity of noun stem and gender is marked by the unproductive declensional types, more or less clear traces of which survive everywhere ⁴⁹).

Many problems may be expected to emerge concerning the relation of noun stem and gender. One such problem is the identity of the nominative singular ending (-a) in the -ā- declension and that of the nominative and accusative plural in the -o- declension system, which was first discussed in detail by J. Schmidt ⁵⁰) and recently by others. These questions, however, do not directly touch upon the origin of gender, although they can perhaps provide some useful clues.

One thing stands out clearly from what has been said, namely, that gender did not spring forth simultaneously throughout Proto-Indo-European, it did not come about in a uniformly identical way and in consequence of identical causes. If nothing else, this shows that the category cannot be treated in generalized terms even within one language family. It seems highly probable that the neuter and the non-neuter gender (*genre animé* – *genre inanimé*) emerged due to syntactic motives, while the masculine and the feminine got separated as a result of morphological-semantic causes.

(to be concluded)

MTA Nyelvtudományz Intézete,
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FODOR ISTVÁN

⁴⁹) Concerning this see Skalička, V.: *Vývoj české deklinace*, Praha, 1941 differently in Rocher, K.: *Gramatický rod a vývoj českých deklinací jmenných*, Praha, 1934.

⁵⁰) Schmidt, J.: *Die Pluralbildung der indogermanischen Neutra*, Weimar 1889.

PROBLEMS OF PHONEMIC INTERPRETATION II. * LONG VOWELS IN A TONE LANGUAGE

Summary

What is to be considered as a long vowel in a tone language? After the meaning of the phrase that "long vowels occur" has been delimited, the concept of the syllable in a tone language is discussed. The Yoruba phonetic syllable appears to be inadequate as a frame to establish the phonemic status of the long vowels. With the aid of the concept of "basic word", the many instances of long vowels in Yoruba registered by others are investigated. It is concluded that all of them are to be structurally interpreted as sequences of two short vowels, and that thus in the phonemic sense defined Yoruba can be said to have no long vowels.

"All vowels occur both short and long". "Long vowels occur in Yoruba, though there do not seem to be a large number of words distinguished by length of vowel alone". These are two quotations from studies on the vowels of Yoruba, a tone language spoken by about four million people in West Africa.

D. Olmsted, from whose study on "The Phonemes of Yoruba" ¹⁾ the first statement was taken, cites several examples in which he contrasts the same vowel in the same environment, and in which the only difference consists in the length of the vowel. Thus he mentions /ti/, 'lock it' as an example of a short /i/, and /ti:/, 'weaving movement' (e.g. that of a drunken man), as the corresponding long one – both words with the tonal glide indicated as "low rising contour".

The second quotation has been taken from Ida Ward's "Introduction to the Yoruba Language" (1952) ²⁾. Among the examples she gives there is not one pair distinguished by length alone; the only case where one might see a "distinction by length" would be case 1): *ó jẹ é*, 'he eats it', as contrasted with *ó jẹ*, 'he eats', not cited by Ward in this connection because the non-progressive form of the verb would hardly occur without an object ³⁾.

*) See also: *Lingua* VII, 1958, p. 356–366.

1) *WORD* 7, 1951, p. 245–249. The quotation is from p. 246.

2) p. 14.

3) In this paper Yoruba spelling is used, which is fairly phonetic. An *n* after a vowel indicates nasalization of that vowel: *fun* is [fũ]; *ẹ* stands for [ɛ], *o* for [ɔ]; *ɟ* for [ʃ]. Standard Yoruba is that spoken by newsreaders on the radio.

The interesting thing about these two quotations and examples is, that what is cited by Olmsted as a *short* vowel (/ti/, 'lock it') would fall under Ward's *long* vowels (tì i, 'lock it'), the object pronoun being "a lengthening of the final vowel of the verb" (Ward, p. 14). The two authors are not clear on this question of length, and the reason of the obscurity is that they do not sufficiently define their terms.

What is to be understood by the term "occur" ("long vowels occur")? Moreover, and of fundamental importance: *what is to be considered a long vowel in a tone language?* Should it have one and the same level tone throughout or can it have a gliding tone? Should it be in one syllable or can it be spread out over two syllables? If it can cover two syllables, should these two syllables be within one word or can we still speak of a "long vowel" when its second part belongs to the next word in the text examined?

All these points should be settled before the information that "long vowels occur" or that "all vowels occur both short and long" in a certain language, can have any linguistic value. What to do with a statement as Ward's on p. 14: "Long vowels are found in the following cases: 1) When the 3rd person *singular object pronoun follows* the verb. This is a *lengthening of the final vowel of the verb*. Note that it is generally on a different tone from *that of the verb*." (Italics BS) Or what with Olmsted's distinction of five "tone phonemes" in /ro/: 1) low tone, 'to stir'; 2) mid tone, 'to hoe'; 3) high tone, 'to sound'; 4) low-mid rise, 'stir it'; 5) mid-high rise, 'hoe it'; whereas one could get along with only three tone phonemes for the interpretation of these five cases: hig, mid and low. No. 4) is *ro* 1) plus the object pronoun which shows complete vowel harmony and is, very regularly, on a mid tone after the low tone verb. No. 5) is *ro* 2) with the same object pronoun on the usual high tone after mid-tone verb.⁴⁾

For tone indication I use ' for high tone, ` for low, no symbol indicates mid tone. These two symbols suffice to indicate "glides" on long vowels: áá – high-mid; aâ – mid-low; ââ – high-low, etc. For short vowels ^ indicates high-low, ^ low-high; for the other glides I use Ward's tone-marks over the vowel:] for low-mid; [mid-high; ⊃ high-mid; ⊂ mid-low. If the horizontal strokes in the latter signs are viewed as representing imaginary horizontal lines indicating the highest and the lowest pitch between which the various tones rise and fall, these symbols will not be difficult to remember.

⁴⁾ See the discussion of this group below, pp. 51, 52.

In Ida Ward's book this is the only case in which her "long vowels" transgress the boundary of words (see below, p. 50), but Olmsted systematically uses the term "occur" with regard to a wider frame than the word unit, viz. the utterance. Only thus can he arrive at such statements as: "Two- and three-vowel clusters are found in all positions; four-vowel clusters occur medially and finally" ⁵⁾, and cite as examples: /mokeuéfô/, 'I write you' – which contains at least four words, spelled in Standard Yoruba: *mo kòwé fún ọ*, in which *kòwé* is a contraction of *kò*, 'write' and *iwé*, 'letter': 'I write-letter to you' – and /ouoeuê/, 'paper money' – in Standard Yoruba: *owó ewé*, 'money – leaf', where the spelling rightly indicates the consonantal quality of the [u].

In such a wide setting it would not be difficult to find vowels of treble and quadruple length: *mo ti rí i iyá!*, 'I have seen him, Mother!'. Besides, if the two vowels in *rí i* form a long vowel, why not call the last two vowels in *mo rí ọ*, 'I see you', a "diphthong", and the middle ones in *mo rí ọ Adé*, 'I see you, Ade', a "triphthong"?

It is this wide setting which causes Olmsted to arrive at his large inventories of both phonemes and tonemes – of the latter he is compelled to assume as many as nine. Such complicated inventories of Yoruba, resulting from a neglect of both its word- and its syllable boundaries, show once again that the phonemic description of a language, if it is to answer the fundamental requirement of simplicity, cannot do without these concepts. An attempt will be made to show that a use of them will bring about a decrease in both inventories and a simplification in the respective systems.

It will be necessary to start with a few definitions.

Occur. The expression that something *occurs* in a language can of course be used in two senses. First of all there is the general, the phonetic, sense of: it is to be found, it can be heard in the language. This seems to be the sense in which Olmsted uses the term, although he uses it in what is meant to be a phonemic picture of Yoruba when he gives as examples only "minimal pairs". Yet from the same phonemic point of view this is difficult to combine with the statement on the previous page that "Yoruba has seven vowel phonemes": if the long and the

⁵⁾ D. Olmsted, *Comparative Notes on Yoruba and Lucumi*, Language 29, 1953, p. 159.

short vowels are two different sets of phonemes capable to differentiate between two otherwise similar words, as Olmsted's examples suggest, Yoruba should be said to have fourteen vowel phonemes. Ward is aware of this difference when she says: "Long vowels occur in Yoruba, *though there do not seem to be a large number of words distinguished by length of vowel alone.*" (Italics BS)

In the present discussion the expression "long vowels occur" will be limited to its narrower, phonemic sense of: they are to be found *as phonemes*. In which a *phoneme* is taken in its usual sense of: a sound in a word whose replacement by another sound of the same language would change the word into another with a different meaning or into a meaningless soundgroup. This means that, if in a certain language when it is being spoken, a word ending in a vowel is followed without a pause by *another word* beginning with or consisting of that same vowel so that one long vowel is heard, this fact alone does not entitle us to say that "long vowels occur" phonemically in that language.

Syllable. Besides occurrence within one word, it must be a second requirement that, if it is to have phonemic status, a long vowel should be in *one syllable*, in which the term "syllable" is taken in the phonetic sense of "a peak of sonority with half the valley on either side"⁶). In other words, the long vowel must present only one peak of sonority: if it should have two, it would form two syllables and be considered as a sequence of two short vowels.

After this the dealing with Yoruba "long vowels" would be plain sailing – if there was not the difficulty of the tones. If we have a "long vowel" in a word, with a high tone on its first part and a mid tone on its second, have we to do with one peak of sonority or two, one syllable or two? It is often difficult to hear. Take for instance the Yoruba word for 'excellent': *dáradára* (*dára* – 'good'). The *r*'s in this word are often

⁶) It might be added: "as observed by the human ear." That this is an essential condition has been proved by recent experiments in which the instrument clearly indicates three peaks of sonority in French *civil* and four in Italian *orso* whereas the ear somehow registers only two peaks in each. (Iván Fónagy, *Kleiner Beitrag zur Silbenfrage*; Zeitschrift für Phonetik und Allgemeine Sprachwissenschaft Bd. 10, 1957, p. 269–278.): "Es ist ebenfalls keine Seltenheit, dass ein einlautendes *s* oder *f* einen, von beiden Seiten durch ein Lautstärkeminimum abgegrenzten Gipfel bildet ... Ein energisch gesprochener *s*-Laut konnte mitunter selbst die benachbarten Vokale überragen" (Example: German *progressiv* shows three peaks, but the highest is on the *s* (p. 275)).

dropped: *daadaa*. In fast speech the vowels are shortened but keep their tones, and it sounds as if the word has only two syllables. The word does not always occur in one of these three extreme forms, however; its vowels may present all the different intermediate stages of length, according to the rate of speech. At which stage are we to assume a dissyllabic word instead of a four-syllabic one? More particularly: does the second form, *daadaa*, contain *two long* vowel phonemes each with a *glide* as toneme, or *four short* vowels each with its *level* toneme?

Klingenheben, in his paper on the subject, seems to find no problem here, not even when the "long vowel" is shortened and others (e.g. Westermann) consider it a case of contraction and loss of vowel. In Ewe *ká àsí*, 'touch with the hand', which has become *kásí*, Klingenheben says: "... hat sich ... in Wirklichkeit überhaupt kein linguistischer Vorgang abgespielt, sondern die einzige Änderung besteht in einer Veränderung der Orthographie. *Verrät doch schon das doppelte Tonzeichen, dass die Zahl der Silben sich nicht wirklich geändert hat.*" ⁷⁾. Even when the "lost vowel" is different from the remaining one, Klingenheben refuses to consider it as loss of syllable; in cases such as *dé así* > *dásí*, *káè* > *kê*, "handelt es sich nicht um eine Kontraktion, da die Silbenzahl unverändert bleibt, sondern um blosse Assimilationen von Sonanten, im ersten Beispiel um eine total regressive, im Zweiten um eine Reziproke." (p. 17, italics BS)

But if two tones on a vowel automatically make two syllables, what to do with the assimilation phenomena in Yoruba when a word beginning in a high tone changes this high tone into a glide low-high when it is preceded by a low tone – and vice versa? Is one to understand that these words suddenly develop a new syllable under such circumstances? (Example: *měta*, 'three; *ènià měta*, 'three people').

On the other hand, if two different tones on one vowel sound make two syllables, should not two tones of the same pitch on a vowel sound also make two syllables? Why should in that case a different criterion be applied by Klingenheben in the "non-syllabic vowel" separating the two "vocalic vowels" – a criterion which does not seem to be required for the other cases? Klingenheben adduces examples from Vai (Liberia). The Vai syllable, he says, is CV, with short V: "das Vai kennt phonolo-

⁷⁾ August Klingenheben, *Zum Problem der Silbe in Afrikanischen Sprachen*. Afrika und Übersee Bd. XXXVII, 1952, p. 17.

gisch nur *eine* Lautdauer" (p. 10). In the sequences of $a + a$, $e + e$, etc., the "C", the syllable valley, is indicated by ' or $\hbar$ but "was wirklich gesprochen wird, wäre eher als leiser Einsatz zu bezeichnen der zwischen ihnen stehende Konsonant ist ebenfalls der gleiche, nur druckschwächere und daher vom Silbenakzent nicht getroffene und unsilbische Vokal, so dass sich als phonetisch die Lautfolgen $\bar{a}\bar{a}\bar{a}$, $\bar{e}\bar{e}\bar{e}$, $\bar{e}\bar{e}\bar{e}$ und $\bar{o}\bar{o}\bar{o}$ ergeben" (p. 12) – in which $\bar{}$ indicates the "consonantal vowel".

It is not clear from Klingenhoben's paper whether the last criterion of "non-syllabic vowel" is supposed to be automatically present (whether we hear it or not) where there are two tones on a vowel, so that it should be supposed to be the only means to mark off the syllables in a tone language, or whether different criteria are to be applied in different cases.

However this may be, one thing is clear: the concept of the syllable as such will not be of much help to form an opinion on the phonemic status of long vowels in a tone language like Yoruba. We must resort to additional criteria.

Such an additional criterion is to be found in the *morphemes*, or rather, in the *basic words* from which the morphemes are taken. With the aid of the morpheme it will be possible to arrive at a more clearly outlined concept of the syllable in the Yoruba word, and via this at a structurally satisfactory interpretation of its long vowels.

Basic word. Basic words are the monosyllabic words of the structure CV and the dissyllabic words of the structure (C)VCV not derived from them. These words, with or without prefixes, can enter freely into larger word units as morphemes, in which process they may undergo various degrees of assimilation: loss of consonant, assimilation of vowel, loss of vowel with or without loss of tone. These assimilations are the same as those that take place in the syntactical juxtaposition of words in the sentence. (Examples: *aldààkóso*, 'president, boss', from *a* (noun-forming prefix); *lí*, 'to have'; *àkóso*, 'control, government' (*à*, noun-forming prefix; *kó*, 'to gather together'; *so*, 'to bind'). Here the high tone of *lí* and the low tone of the following *à*- have been preserved in the resulting long *-áà-*. *Sókè* or *sókè*, 'upwards', from *sí*, 'towards', and *òkè* 'top'. *Bímọ*, 'to have a child', from *bí*, 'to give birth to' and *ọmọ*, 'child'. *Àárọ̀*, 'morning', longer form: *àwúrọ̀*, *òwúrọ̀*) ^{7a}).

^{7a}) Etymology uncertain: *òwú*, 'cotton'; *rọ̀*, 'to become soft, to dissolve'. Cf. *òwúsúwúsú*, 'fog' (*sú*, 'to break forth'), *ọjó rọ̀*, 'evening' (*ọjó*, 'day').

Inversely, even as morphemes within a larger word unit, these basic words retain a large degree of independence. For instance, even within the longer word unit the morpheme can have its own object or qualifying adjunct as a separate word in the sentence. *Gbádùn*, 'to enjoy', is composed of *gbó*, 'to perceive' and *adùn*, 'sweetness' (*a*, noun-forming prefix; *dùn*, 'to be sweet', 'to be pleasant'). 'I enjoyed it', therefore, is not *mo gbádùn un* with *object* pronoun 3rd person, but *mo gbádùn rẹ* with *possessive* pronoun 3rd person: 'I perceived-sweetness its', in which the possessive pronoun qualifies only the part *-(a)dùn* of the word *gbádùn*, 'to enjoy'. Similarly, in the group *aláwọ̀ bàbà*, 'person with copper-coloured skin' (*a*, noun-forming prefix; *lí*, 'to have'; *awọ̀*, 'skin'; *bàbà*, 'copper') the adjective *bàbà* qualifies only the part *-awọ̀* of the word *aláwọ̀*, 'possessor of skin'.

Thus the behaviour of morphemes in the sentence is in many respects undistinguishable from their behaviour as independent words, and the importance of the basic words in the whole structure of the language is evident. At times it seems to be merely the spelling and the translation which join a number of such basic words together as "morphemes" into one "compound word". Instead of polysyllabic compound words, therefore, it should rather be their morphemes or the *basic words* from which these have been taken, that should form the frames within which we are to look for long vowels.

If we do so, several facts emerge.

First of all, the structure of the great majority of Yoruba basic words is CV or (C)VCV. When they occur in isolation (cf. *mẹ̀ta* above), for instance in an enumeration, they contain short vowels each with a *single tone*: high, mid or low.^{7b)}

Secondly, the majority of Yoruba "long vowels" have *two different tones*:^{7c)} often interpreted as a "glide" (Ward, Olmsted).

Thirdly, contrary to the morphemes of the form CV, the "mor-

^{7b)} Only high after low and low after high tone are realized as glides: *díé* [díé], 'little' (See Fn. 16 p. 54 below). That this glide is a high (resp. low) tone phoneme (toneme) appears from the tonal behaviour of a following syllable with the opposite tone, which behaviour is the same as when that syllable is preceded by a low tone proper: it is realized as a glide: *díédíé* [díédíé], 'little by little' (cf. *ènià mẹ̀ta* above, p. 46).

^{7c)} Apart from low-high and high-low also low-mid, mid-high, high-mid and mid-low occur. Cf. *dáádáa* above, p. 46).

phemes" of the form "C + long V" which occur in many words will hardly ever be found to occur as words by themselves, so their meaning is obscure.

Fourthly, in most of the cases of such unidentifiable morphemes with long vowel within a word, there is a longer and/or older form of the word with two short vowels – with or without intermediate consonant, which often solves the unidentifiable "morpheme" into two identifiable ones: *káalé* = *ékú alé*, 'good evening'; *báalé*, 'master of the house' = *babá* ('father') -*ilé* ('house'). This agrees with Klingenberg's examples in which most of the long vowels also seem to owe their existence to the loss of an intermediate consonant or a combination of two short vowels.⁸⁾

Fifthly, this basic structure CV with short V agrees with what is presented as a general characteristic of the syllable in most West Sudanic languages.^{8a)}

In view of these facts it seems justifiable to assume that for Yoruba, too, the syllable is of the form CV, with the V always short and with a single tone, high, mid or low. As regards the "long vowels": though their etymology may sometimes be obscure, yet the overwhelming majority of analyzable cases, in addition to all the other factors just mentioned, suggest that phonemically speaking Yoruba has no long vowels and that, where they occur, they are to be interpreted as sequences of two short vowels belonging to two different syllables^{8b)}.

The various instances of "long" vowels observed in Yoruba by others will now have to be examined from this point of view to see whether the interpretation can be maintained or not.

As Olmsted's categoric statement that all vowels occur both short and long is not further specified, we turn to Ward's more detailed account of the cases in which she has found long vowels (p. 14 ff.). To facilitate reference the full list follows here:

1) "When the 3rd person singular object pronoun follows the verb. This is a lengthening of the final vowel of the verb. Note that it is

⁸⁾ "Nicht eine Elision, sondern eine Assimilation des Konsonantischen *l* an seine vokalische Umgebung": *kala* > *kāaa* (written *kaa*).

^{8a)} D. Westermann, *Die Westlichen Sudansprachen und ihre Beziehungen zum Bantu*, 1927, pass.

^{8b)} Apart from the cases of "affective lengthening", see below (O!).

generally on a different tone from that of the verb: *ó rí i*, 'he sees him'; *ó jẹ é*, 'he eats it' The tone of the verb plus object is a glide."⁹)

2) "Vowels on a rising tone are somewhat long": *orẹẹ*, 'friend'; *iyàa*, 'mother'.

3) "Vowels on a falling tone are somewhat long": *bẹẹ ni*, 'yes' (litt.: 'so it is'); *ó máa nìlọ*, 'he usually goes'.

4) "The *o* used at the end of a salutation is long": *ẹkáǎrò oo*, 'good morning'.

5) "Long vowels occur in the final syllable of a noun when this is followed by a possessive adjective": *omọ̀ọ̀ mi*, 'my child'.

6) "Long vowels occur in certain grammatical constructions"¹⁰): *ó fẹ̀ẹ̀ lọ*, 'he wants to go'; *wáa jẹun*, 'come (and) eat'.

7) "Long vowels occur when a syllable or sometimes merely a consonant has been dropped, i.e. in an abbreviation of some form which involves such a loss": *ẹ̀ẹ̀kẹ̀*, 'cheek', from *ẹ̀rẹ̀kẹ̀*; *Yòòbá*, 'Yoruba', from *Yorùbá*. "In all cases the original tone pattern of the full word is retained".

Several other items could be added to this list or to its examples, but it is sometimes difficult to know under which heading to insert an additional case found. The list covers a mixed variety of phenomena whose division into seven different groups has no structural foundation. The collection will become more surveyable if the *word* is taken as basic unit for the analysis, and if the instances of "long" vowels are divided into three main groups:

1. Long vowels = *two words*
2. Long vowels *within* a word
3. Long vowels *on the word-boundary* as junction phenomena.

1. Long vowels = two words

Verb plus object pronoun third person singular

The first case of Ward's list is that of the verb plus object pronoun third person singular. Ward's own wording of the case shows that she is

⁹) The examples are Ward's in the present writer's transcription. The tones are indicated as registered by the present writer, which in some cases is slightly different from the tones registered by Ward; but long vowels are heard in all cases.

¹⁰) The various grammatical constructions have been described and analysed in detail by E. C. Rowlands, *Types of Word Junction in Yoruba*. Bulletin School Oriental & African Studies, London, Vol. XVI, 1954, pp. 376-388.

not sure whether the vowel-sound expressing this pronoun is part of the preceding verb or a word in its own right.

Seeing that all the other object pronouns are words in their own right, it might be expected, to begin with, that the 3rd pers. sing. would be one too. If, further, we consider the object pronouns themselves, there is even more to be said for this interpretation. All the other object pronouns correspond to the final syllable or vowel of the dissyllabic strong forms of the subject pronouns:

Stressed <i>I</i> :	<i>èmi</i>	–	Object form:	<i>mi</i>
,,	<i>you: ìwọ</i>	–	,,	: <i>ọ</i>
,,	<i>we: àwa</i>	–	,,	: <i>wa</i>
,,	pl. <i>you: èyin</i>	–	,,	: <i>yin</i>
,,	<i>they: àwọn</i>	–	,,	: <i>wọn</i> .

Now the stressed form of the subject pronoun 3rd person sing. is *òun*, in which only the first vowel is clearly heard; the rest, the second syllable, is a sort of indefinite nasalized murmur with an *u*-like quality, the whole sounding something like: [òũ], or [òŋ]. It is very tempting to venture as interpretation of the object pronoun that it is originally this indefinite nasalized murmur sound, adapted to the vowel of the preceding verb, showing the vowel-harmony so frequent in Yoruba in other combinations as well ¹¹). Examples: *ó rí i*, 'he sees it'; *ó rà a*, 'he buys it'; *ó wò o*, 'he looks at it'.

An objection to this interpretation might be that, if the relation of the 3rd pers. obj. pron. to the stressed subject pronoun [òũ] were analogical to that between the other sets of pronouns, its adapted vowel should be *nasalized*. On the other hand, the weak forms of the subject pronoun 3rd pers. sing.: *ó* (before syllables with close vowels), *ọ* (often before syllables with more open vowels), have no nasalization left in them either, although in *òun* the *ò* is often nasalized. A loss of nasalization in the object pronoun, therefore, would not be entirely impossible¹²).

¹¹) Cf. the nouns constructed with the prefix expressing 'owner of', which prefix consists of the vowel *a* plus original *ní* (alternative form *lí*), 'to have'. The vowels of the prefix show complete assimilation to the first vowel of the noun: *aláta*, 'pepper-seller' (*ata*, 'pepper'); *elére*, 'one given to merriment' (*ere*, 'play'); *olómi*, 'water seller' (*omi*, 'water'). (Note the preservation of the high tone of *ní* (*lí*) in the compounds).

¹²) Always assuming that *ó* is indeed the weak form of *òun*, just as *a* is that of *àwa* and in some cases of *àwọn*, and *ẹ* that of *ẹyin* (with frequently nasalized *ẹ*).

The interpretation of the added adapted vowel as the second syllable of *òun* with dropped nasalization would enable us to say that this instance of "long vowels" does not exist from a phonemic point of view. Even the term "lengthened vowel" (of the verb) would not be right as we have to do with different words. Assimilated *o* in *ó gó o*, 'it tires him', is no more a lengthening of the vowel of *gó* than original *o* in *ó gó o*, 'it tires you', is. (See also p. 64 fn. 27). Moreover, the fact that the two vowels are always on different tones, *the pronoun changing its tone to make this possible*¹³, is a strong point in favour of this interpretation. It is perfectly correct that (although the newspapers are not always consistent in this respect) Standard Yoruba spelling writes the second vowel as a separate word.

2. Long vowels within a word

a. Affective lengthening

Of the long vowels within a word, no. 4 of Ward's list seems the easiest to deal with. The lengthening of the *o* in Yoruba exclamations is no more remarkable than the lengthening of the English vowel *o* in exclamations: it can be simply explained as "affective lengthening". The lengthening in the Yoruba *o* is even easier to account for than that in its English counterpart, as it is not followed by the rest of the exclamation (as mostly with English *o!*), but stands at the end where it is more likely to be drawn out. It often seems to serve as a kind of "amplifier" to make the call more audible. People add it to proper names when calling their friends: "*Ade-o!*", "*Dada-o!*", and it is put at the end of whole utterances: "*Èkú oḍún-o!*", 'Happy New Year!'; *Mama mi-o!*, 'Mother!' (litt. Mother my *o!*) Even English texts are transformed into this construction; I have seen the following inter-punctuation: *Help me o, God!*, for which of course the very familiar sound-group [mi: o] is partly responsible (Cf. the previous example).

When asked after the meaning of this *o* – because the same greetings and calls may also occur without it – Yoruba people find it hard to explain. The farthest they are able to get is that, when you add the *o*, it sounds more as if you solicit an answer. Which would seem to put it

¹³) After high and low tone verbs the object pronoun has mid-tone, after a mid-tone verb it has high tone: *mó fẹ́ e*, 'I want it'; *mó rà a*, 'I bought it'; *mó bí í*, 'I ask it'. This rule applies to all the object pronouns.

in the same category as English *o!*, though far less emotional. The answer to such a call is often just an acknowledgement, a sign that you have heard the call or good wish, and consists in turn in a mere *o!*, drawn out long, on a level mid tone. It is evident that this length is non-phonemic. Instead of "long vowel", the term "lengthened vowel" would therefore be more appropriate ¹⁴).

The same kind of lengthening may be said to occur in such interjections as *ẹhẹẹn* (signalling approval, agreement), which, moreover, contains the "extragrammatical phoneme" [ɛ̃] ¹⁵), and in such onomatopoeic adverbs as *fin*, 'deliciously', qualifying *dùn*, 'sweet'. Even the lengthening in the final vowel of *òrẹ*, 'friend', *ìya*, 'mother' (Ward's case 2) might be partly due to such affective lengthening (but see no. 3a below).

b. Elision

If the long vowels described just now were the result of a "stretching" of one vowel, those now to be reviewed are the result of the opposite process, the combination of two short vowels into one longer one. This is Ward's group no. 7: elision, either of a whole syllable or merely of a consonant, resulting in a succession of two vowels. It will be seen that the majority of the other instances of long vowels in the list can be interpreted in this way, viz. most of Ward's nos. 2, 3, 5 and 6.

There are numerous examples to show, as was said before, that "the tone of the syllable omitted is retained and combined with the tone of the next syllable" (Ward, p. 15). The term "combined" means probably that the two tones form a glide: there is no clear break between the two vowels and therefore the pitch glides from the tone of the first vowel to that of the second. From Ward's examples may be taken:

otitọ [_ ʃ̄ _] > *ootọ* [_ ʃ̄ _] 'truth' (tonal indication by Ward)

daradara [_ - - -] > *daadaa* [ʌ̄] 'good' (tonal indication by Ward)

owuro [_ ʃ̄ _] > *aaro* [_ ʃ̄ _] 'morning' (tonal indication by Ward)

koriko [_ - -] > *kooko* [ʌ̄ -] 'grass' (tonal indication by Ward)

¹⁴) For the sound itself, its quality, see B. Siertsema, *Some Notes on Yoruba Phonetics and Spelling*. Appearing in *Bulletin de l'Institut Français d'Afrique Noire*, Série B, 1958.

¹⁵) Cf. B. Siertsema, *Problems of Phonemic Interpretation I, Nasalized Sounds in Yoruba*. *Lingua* VII, 4, 1958.

Ward spells these words with double vowels and that is what their "long vowels" are ¹⁶). Structurally, what has happened is that the consonant has been dropped; the vowel remains but is then assimilated to the preceding vowel. The term "a syllable has been dropped", therefore, is not a very happy one.

The double vowels, however, do not necessarily have the *length* of two vowels; the length may be reduced to that of about $1\frac{1}{2}$ or even to that of one syllable (Cf. the example of *dáadáa*, 'good', above, p. 46). But the comparative shortness of the Yoruba "long" vowels would be no obstacle for their being interpreted as *two* vowels. They will always retain the two tones *until the shorter form is no longer recognized as such* and becomes *a separate word in its own right*. Then, indeed, the two vowels may merge into one *and this is shown by the loss of the double tone*. I have interesting evidence of this in a recording of Ward's Text no. VIII, pp. 220, 221. I have had this text read by different people, all speakers of Standard Yoruba, but of different social status and different degrees of education. In this text there occurs the group: *aláwọ̀ bàbà*, 'people with copper-coloured skin', litt.: 'owner-of-skin copper-coloured'. Most of my readers read the last word as indicated by Ward, with two short low-toned syllables: *bàbà*. But two of them, a school-teacher and a university student, read *bàabà*, with a low-mid "glide" on a long first vowel. On my question why they read this word with a long vowel and with these tones, the answer was: "I thought it came from *bálábàlá*: 'bright' "!" This experience offers considerable support to Klingenberg's assumption that two tones on a vowel indicate two syllables ^{17a}).

¹⁶) Moreover this spelling allows of a clear phonemic indication of the different combinations of tones occurring: *òótó*; *dáadáa*; *àarò*; *koóko*. The present Yoruba spelling with a single vowel with $\sim$ over it is inadequate. As regards the glides $\lfloor$ and $\lceil$ indicated by Ward for short vowels: phonetically, the realisation of a low tone after a high tone and inversely is indeed a glide towards the second tone. But it is not, as Ward indicates, a glide down to or up to the second level, see *òtító*, *òwúrò* above. What I hear in all cases is rather a glide from (i.e. starting at) the first level: the second level is not always reached. In Ward's "narrow" transcription this would be better indicated by $\lceil$ instead of $\lfloor$, and $\lfloor$ instead of $\lceil$. In my "broad" transcription suggested for printing these glides are not marked, just as the difference between clear and dark *l* is not indicated in a "broad", phonetic, transcription of English. See the discussion of glides in the writer's paper on *Yoruba Phonetics and Spelling*, Cf. Note 14 above.

^{17a}) That there are various stages in this development is shown by the next example (p. 55 fn. 17^b).

There remain the cases 2) and 3) of Ward's list: vowels on a rising and those on a falling tone. In view of the foregoing these cases should not offer too much difficulty. It seems justifiable to interpret the rising and falling tones as combinations of low + high and high + low tone respectively. As in the cases of elision already dealt with, in these groups too, many forms with short vowels separated by a consonant are to be found side by side with the ones with the "long vowel" and the gliding tone. For *bèni*, 'yes', 'So it is', e.g., Crowther gives *bẹhẹ ni* (*Grammar and Vocabulary of the Yoruba Language*, 1852). By lack of etymological research and records and by lack of consciousness, on the part of the speakers, of this relation between what to them may be two different words, the longer form may be looked for in vain sometimes, but the persistent investigator will often come across it the next day. Yet, as long as the longer form has not actually been found with the same meaning, the question will remain whether one has to do with two different pronunciations of the same word or with two different words ^{17b}). And this gives rise to the interesting question whether, on the ground of the comparatively few unanalyzable cases one is justified to say that Yoruba has long vowel phonemes and phonemic tonal glides.

The analysis into two syllables does not apply to the long vowels in Ward's examples *òrẹẹ*, 'friend', *ìyàa*, 'mother' and several others which are represented as having final vowels "on a rising tone". It is true that the two words mentioned often occur with *mi* as forms of address, and then present the lengthening described in no. 3a below (elision of *tì*). It is this lengthening which causes the addition of the rising tone and not, inversely, as one might think from Ward's wording, the rising tone which lengthens the vowel. If *ìya* is used without the possessive pronoun, as I have heard it called out to an elderly woman *not* the

^{17b}) As an illustration may serve *aádùn*, 'parched corn, ground and mixed with oil and honey'. *Dùn* = 'to be sweet'; *adùn* therefore = 'sweetness', and then there is the form *aládùn*, 'that which has sweetness' (see Fn. 11), used as an adjective: 'sweetened'. The investigator is quite ready to consider the first word, *aádùn*, as identical with the last with elided *l*. Informants, however, insist that there is no connection and that *aádùn* is "a word by itself". This contradicts the very striking previous example of *bàbà* and the conclusions drawn from it; unless one assumes that the development *aládùn* > *aádùn* > *adùn* is still in a transitional stage, or that *aádùn* must come from a different compound.

speaker's mother, it has *short a*, though even there, in passing from low *i* to mid *-ya*, the tone necessarily *glides* up. Also when followed by, e.g., the definite article *náa* these words have short vowels.

3. Long vowels on the word-boundary as junction phenomena

a. Noun plus possessive pronoun

As said above, the interpretation of the long vowel as two vowels with elision of an intermediate consonant can be shown to apply to most of the other cases of Ward's list. The clearest case is that of nouns plus adjectival possessive pronoun.

The independent forms of the possessive pronouns ('mine, yours, etc.) are: *tèmi*, *tirè*, *tirè*, *tiwa*, *tiyín*, *tiwón*. The first syllable of these words is dropped in the adjectival form, "the final vowel of the noun is lengthened, in some persons on the same tone, in others on a different tone", and "the glide in all cases has the tone of the elided first syllable" of the pronoun: *iyàà mi*, 'my mother'; *iyàà rẹ*, 'your mother'; *iyaa rẹ*, 'his mother'. (Ward, pp. 64, 65).

This suggestion that the "dropped" first syllable of the pronoun has been that of the *independent possessive pronouns*: *ti-*, seems to be the obvious analysis of these forms, *ti* being the word expressing any relation of possession or belonging to (*tèmi* < *ti èmi*: 'of me'; *tiwa* < *ti àwa*: 'of us', in which, contrary to the first pronoun, the vowel and tone of *ti* have been preserved). It seems more probable than Rowlands' attempt to connect the adjectival possessive pronouns to the personal pronouns direct, of which attempt he says himself: "but the correspondences are by no means regular" ¹⁸). Besides, if the lengthening represented the first syllable of the personal pronoun, it would have low tone in all persons (cf. p. 51 above). Instead, it is always on the same tone as the first syllable of the corresponding independent possessive pronoun ^{18a}): low in the first two, mid in the others.

The frequent mid-tone glide of nouns in genitive relation, too, may well represent *ti* (*bàtāa Bóládé*, 'B's shoe'). It could all be described as

¹⁸) See Note 10, p. 50 above.

^{18a}) The first two of these have kept the vowel and tone of the first syllable of the personal pronoun with elision of those of *ti*; all the others present the reverse process: *ti èmi* > *tèmi*, *ti irẹ* > *tirẹ* (*irẹ* is sometimes used as a form intermediate between *iwo*, which may sound derogatory, and *eyin*, which is very polite), *ti irẹ* (origin obscure) > *tirẹ*, *ti àwa* > *tiwa*, *ti eyin* > *tiyín*, *ti àwón* > *tiwón*.

just another instance of *elision* as analysed in the discussion of Ward's case 7) above: the syllable *ti* loses its initial consonant in the close junction with the preceding noun. Then its vowel is assimilated to the preceding one of the noun. That here, too, the length of the two vowels varies in people's pronunciation is shown by Ward's notes on Lagos pronunciation of these groups (p. 69).

Phonetically, there is in the case of the possessive pronouns an interesting glide-phenomenon in the speech of non-Lagos people, which might serve to corroborate this interpretation of the "long" vowel of the groups. The tone of the assimilated vowel, which vowel so to say has "changed words" and now seemingly belongs to the preceding noun instead of to the pronoun, extends in a curious way into the consonant of the pronoun, a voiced continuant in all cases. Therefore the above transcription *ìyaà mi* is not quite what is said, *ìyaà m̐i* would render it more accurately. What Olmsted says about the Yoruba demonstrative pronoun *yi* applies here too: "It is only as a result of our convention of writing tones in connection with "vowels" and not with "consonants", that we are likely to ignore the pitch coincident with [y]" (*The Phonemes of Yoruba*, p. 248). In this case we are likely to ignore the pitch coincident with *m*, *r*, *w* and *y* (*m̐i*, *r̐e*, *wa*, *w̐on*, *y̐in*). Ward has noticed the extra tone (see above) but is not certain on which sound precisely it is to be pronounced. In one place she speaks of the lengthening of the vowel of the noun "on the same tone" or "on a different tone", (p. 64), but earlier on she speaks of "the glide *between* the words" (italics BS), saying that "it may be on the same or on a different tone from that of the preceding syllable" (p. 32).

The true representation of the tonal curve can of course only be a line showing variations of pitch merging into each other.

This extension of the tone of the vowel "of the noun" into the consonant of the pronoun can be seen as a hint, however faint, that the pronoun is the word this vowel belongs to. What is more: in the case of shortening of this double vowel, the low tone does not remain on this shortened "long" vowel but the low tone is first heard with the consonant of the pronoun. Cf. Rowlands: "In quick speech the lengthening may not occur, but then the elision of a low tone can be traced". Rowlands indicates this in the pronunciation of the vowel of the pronoun (a low-mid glide), whereas to me it seems to be the *m* where the low tone starts: *om̐o m̐i*, 'my child'.

But whether in these cases of shortening the low tone starts on the vowel or already on the consonant, it does belong to the second word, to the pronoun. This corroborates the structural interpretation of this "long vowel" as two vowels, the second of which is the assimilated first vowel of the pronoun.

b. Groups of two verbs

Ward's case no. 6 is another instance of lengthening of a vowel by the assimilation of a following vowel to it. It is analogous to the cases mentioned, including the previous elision of a consonant. Examples (Ward p. 116): *ó lọọ fọ așọ*, 'she went to wash the cloth'; '*ó fẹ́ fẹ́ jeun*', 'he wants to eat'; *kò șee gé*, 'it can't be cut' ^{18b}). Ward gives the analysis in this case as in the previous one: "It is probable that this lengthening of the vowel takes the place of a prefix of the second verb and that the construction is an abbreviation of: *ó lọ ifọ așọ*; *ó fẹ́ ifẹ́ jeun*; *kò șe ígé*. . . . "In every case the lengthening is on a high tone, and would therefore correspond to the *reduplicated form* which is the verbal noun. . . . I am told that both forms, i.e. with the full verbal noun and with the vowel prefix *i* (on a high tone), may be heard in certain Yoruba dialects. The prefix is frequently written." (p. 116; italics BS).

It is interesting to note that Ward calls the second verb in these constructions a "verbal noun". The prefix of pure verbal nouns, *ì*, is always on a *low tone* ¹⁹), whereas the elided vowel-prefix in the constructions under discussion had a *high tone*. The vowel may well have been *i* too, but its high tone and the fact that in these combinations it occurs side by side with the full reduplicated form of the verb according to Ward, show that this *i* is probably the remainder of the reduplication syllable which is always on a high tone and has the vowel *í* in most cases ²⁰). By means of this reduplication syllable, verbs are made into nouns, but these nouns are more "verbal" in character, so to say, than the verbal nouns formed by means of the prefix *ì*. Remarkable too, is that only the reduplication nouns can be used as adjectives after

^{18b}) Litt. 'not does cutting'.

¹⁹) Cf. *ibá jẹ*, 'the act of dealing with another' (*ì* (prefix), *bá* (help to do) *jẹ* (eat)); *ibímọ*, 'child bearing' (*ì* (prefix), *bí* (give birth to), *omọ* (child)); *ìfojúsi*, 'attention' (*ì* (prefix), *fí* (put), *ojú* (eye), *sí* (towards)).

²⁰) E.g. *jìjà*, 'fighting' (*jà*, 'to fight'); *bìbùjẹ*, 'biting' (*bùjẹ*, 'to bite'); *dídàrà*, 'good', litt. 'being good' (*dàrà*, 'to be good').

another noun *igi gíga*, 'tall tree' or better: 'talling tree'.²¹⁾ In fact, the reduplication syllable forms verbs into something that might be compared to the English *-ing* forms. They, too, can be used both as adjectives (*dancing children*) and as nouns (*I like dancing*), but as nouns they are more verbal in meaning than the nouns formed from the same verb by other means. Thus *ìjà* is related to *jíjà* (*jà*, 'to fight') in the same way as, e.g., *preparation* is related to *preparing*, *laughter* to *laughing*, *employment* to *employing*, etc. The sentence which Ward gives as an example of this verbal noun:

Jíjà rè n'ìgbà gbogbo kò mú àláfíà wa

'His fighting always does not bring peace',^{21a)}

is a clear example, in which the change of *jíjà* into *ìjà* would correspond to English: *His eternal fights* . . .

Thus we have to distinguish two prefixes *i* in the formation of nouns from verbs: 1) *ì*, which forms "nominal nouns", and 2) *i* < *C* + *i*, which forms "verbal nouns". It is the second *i* then, that is assimilated to the vowel of the preceding verb in combinations of two verbs.²²⁾

c. Noun + adjective

It is the more puzzling to find that still a third kind of vowel prefix has presumably been assimilated in certain combinations of noun plus adjective found by Rowlands and discussed in his Section A2²³⁾: *ilée kékeré*, 'the small-boy's house', as opposed to *ilé kékeré*, 'a small house'; *òkọ dúdú*, 'husband of dark ones', as contrasted to *òkọ dúdú*, 'a dark husband'. Here the second part of the lengthened vowel of the

²¹⁾ They are also the only ones possible when stress is laid on the action itself, the way it was performed, rather than on its occurrence. E.g. *Jíjà rè tí fún u ní èbùn*, 'His fighting has won him a prize'. If *ìjà* were used, it would require the word *ona*, 'way, manner' before it: *Ona ìjà rè tí fún u ní èbùn*, 'The manner (of) his fight . . .'

^{21a)} Litt. 'Fighting his at time all not makes peace come'.

²²⁾ The long vowels in the other words usually occurring as a kind of auxiliary with a main verb can probably also be explained in one of these two ways: *le*, *ki* and *ma*, called by Olmsted morphemes or prefixes (potential, negative habitual and habitual respectively), (*Covert (or Zero) Morphemes and Morphemic Juncture*. IJAL 17, 1951, p. 164). *Le* and *ma* are certainly verbs, meaning 'to be able' and 'to do always'; *ki* seems to be a mere negative particle although it does occur as a verb with different meanings ('to load, cram').

²³⁾ E. C. Rowlands, *Types of Word Junction in Yoruba*, p. 377, 378.

first word is always on a *mid tone*. Rowlands assumes elision in these cases: "This extra length can best be interpreted morphologically as the vowel initial of the second nominal. We may say that the Independent form of this type of nominal has a vowel initial which is realized as zero in some contexts and which is always assimilated to the final vowel of the leading nominal in a nominal combination." (p. 377).

This second vowel, then, would either have to be interpreted in the same way as was attempted in the formation of the adjectival possessive pronouns, see p. 56 above, viz. as *ti* +, in this case, a *verbal noun* (reduplication form), or as a special vowel prefix which forms nouns from verbal "adjectives" (the same reduplication forms). Both interpretations, however, meet with grave obstacles. If *ilée kékeré* is analyzed as *ilé ti kékeré*, 'house of small one', we would have the verbal *noun kékeré* (from the verb *keré*, 'to be small'), connected with the preceding noun by means of the preposition *ti*. In this case the group would be a "prepositional genitive group". But: the reduplication forms do not make *nomina agentis* from the verbs, but *abstract nouns* indicating the *action*, either past or present (i.e. they can be translated either as present or as past participles, cf. Note 19 above): *kékeré* = 'being small'.

The second analysis would be: *ilé V-kékeré*, 'house - small one', with the verbal *abstract noun kékeré* turned into a *nomen agentis* by a vowel prefix in the same way as, e.g., the English abstract noun *elocution* is turned into the *nomen agentis elocutionist* by a suffix. In this case the group would be an "appositional noun-group". But there are considerable objections to this second interpretation too: although there is indeed a vowel-prefix on a mid-tone, viz. *a-*, forming *nomina agentis* (see for example fn. 11 p. 51 above), yet the three known noun prefixes form nouns from *verbs*; this one would form nouns from abstract nouns, in their turn made from verbs by reduplication, a rather roundabout way though not entirely impossible.

Conclusive for both interpretations should be, however, that this procedure of turning an abstract noun into one indicating a person is by no means regular; in fact it can be said to be non-existent. It would be difficult to find more cases in addition to the two mentioned by Rowlands. Each new attempt at analogical formations is met by laughter from my informants. They interpret the two cases mentioned as follows.

Ilé kékeré (house - being small) you will find over the door of the

house of a man called *Kékeré*, just as you would find *ilé Latunde* or *ilé* followed by any other name; this group serves as nameplate therefore. It is pronounced with a lengthened *é* in all cases, because otherwise it would mean 'small house', but it cannot be used to indicate the house of somebody who is small or who works as "small-boy" if that person does not bear the name *Kékeré* ²⁴).

This reduces the case to another instance of case 4, with original *ti*, but without the assumption that *kékeré* and similar verbal nouns could be used as nouns indicating persons. *Ilée gígún*, 'house tall' and *ilée títobi*, 'house big', for instance, which in that case should be equally possible to indicate the house of a tall or of a stout person, do not occur: "because *gígún* and *títobi* do not occur as names of persons" my informants say.

Rowlands' second instance of this construction strengthens the thought of non-existence of any grammatical procedure of this kind in a more negative way. *Ọkọọ dúdú*, 'husband of dark ones' ²⁵), is not a generally used formation; it might occur as a special, private idiomatic coinage in the very limited context of the Yoruba code of behaviour within the family-compound, where it is not allowed for a wife to call the brothers of her husband by their names. She invents a nickname for each and might thus call a brother-in-law who has particularly dark wives: *ọkọọ dúdú*; but nobody else would do so. They would say: *ọkọ enia dúdú*, 'husband (of) people being-black'. The reason why it would be possible for the wife to use the construction is hidden in the dark past and one does not know where to look for it. It might probably be compared with an imaginary situation among English speaking people, who might in jest say of a man who always takes particular care in planning and arranging for the future: "He is a [fɔ:sait]". This would be perfectly clear to all those who knew Galsworthy's character of that name, *Forsyte*, who had those qualities (*The Forsyte Saga*, 1921). But it would by no means justify a linguist to conclude that in English abstract nouns like *foresight* can be made into nouns indicating persons

²⁴) This would class the lengthening of the preceding vowel among the cases with adjectival poss. pronoun, where the lengthening was interpreted as original *ti*, which in the third person sing. also had *mid-tone* and which has resulted in a lengthening of the vowel of the preceding noun on a mid tone; see pp. 56 above.

²⁵) *Dark* and *light* or *fair* refer to the degrees of blackness of the skin.

by putting an article before them. "He is a debate" would not do for a person who loves debating, not "She is a poise" for a woman having that quality.

Informants are puzzled when so much interest is shown in this lengthening and when it is attempted to make "strange" constructions. *Ìyáa dúdú*, 'mother of black ones', for instance, cannot be used. Nor would the hut of a particularly black wife ever be called: *ilée dúdú*, nor would the house of a particularly black brother-in-law ever be called such by the wife. "Because *dúdú* is not a name!" is the explanation given.

Apart from the case of proper names, I cannot elicit a single instance in which the lengthening of the final vowel of a noun before an adjective would result in a different meaning. Inversely, the example under discussion is so closely bound up with the situation in which it is used that omission of the lengthening does not result in a change of meaning either:

To the question: if a wife would call her brother-in-law *ọkọ dúdú*, would it not mean 'black husband'? – my informant answered: She would never address him as 'husband' – it would still mean the same.

Next question: If she said *ọkọ dúdú*, would you not think she spoke a funny kind of Yoruba? Would you not laugh at her and say: "Can't you speak properly?"?

Answer: No, it would not strike me as strange.

Question: If she said *ọkọ dúdú*, would you think she came from a different part of the country?

Answer: No, it would not sound strange.

Question: But normally she would say *ọkọọ dúdú*; surely you would hear that *ọkọ dúdú* was unusual? What *would* you think on hearing it?

Answer – after thinking deeply for a while –: I would think that she was in a hurry.!

Having herewith reached the end of the list of long vowels observed by others, we must emphasize first of all that it does not cover all the cases there are. Again and again the student of Yoruba speech comes across new long vowels which ask for a structural interpretation. What causes, for instance, the lengthening of the future auxiliary *ó* when the group 'I shall' is pronounced [ɲó] with consonantal [ɲ]? Is it the loss of

the syllable *í* in this group, which in slow speech has its own high tone before the short syllable *ó*?

There certainly does seem to be a general tendency in Yoruba to preserve the original tonal picture of a word or group at all costs, and to compensate for this purpose the loss of one "tone-bearer" by the doubling of another. The term "double vowel" or at most "lengthened vowel", therefore, should be the term to indicate the Yoruba long vowels. For after the above investigation the statement appears to be tenable that *there are no long vowel phonemes in Yoruba*.

This statement has far-reaching consequences for our conclusions regarding the inventory of phonemes, in particular when confronted with Olmsted's large inventories of 3×7 vowel phonemes (short, long and nasalized), and nine tone phonemes (low, mid, high, low-high rising, mid-high rising, mid rising, high-low falling, mid-low falling, mid falling (*The Phonemes of Yoruba*, p. 247).

We see that the analysis of long vowels into two short ones reduces the vowel inventory by seven items and that of the tones by six: all the rises and falls. It is perfectly true that these glides and many others "occur" in the language; one will even hear Olmsted's "mid-rising" and "mid-falling" glides.²⁶ But they can all be interpreted phonemically as combinations without a break of two tones after elision, or phonetically as other junction phenomena (Cf. *Yoruba Phonetics and Spelling*. See Note 14 p. 53 above).

Spelling

Finally, there is the practical question whether the double tones ("glides") and double vowels ("long vowels") should be indicated in the spelling or not. On this point we might profitably follow and generalize Ward's method and recommendations. She writes two vowels, each with its own tone-mark, when the double vowel belongs to one and the same word. This disposes of all the double tones *within* one word. She also writes a second vowel as a separate word in the case of the third person singular object pronoun, which spelling is also used by Standard Yoruba writing. In the case of verbal combinations, on the other hand,

²⁶ Moreover, sentence intonation may influence the pitch to a great extent (Cf. W. J. Samarin, *Intonation in Tone Languages*, African Studies 11, 1952, p. 80-82).

Ward suggests writing the prefix *i* or an apostrophe (p. 116) in the second word: *kò ʃe ígé* or *kò ʃe 'gé* [kò ʃee gé], 'it cannot be cut'.

The first spelling is open to the objection that the vowel is usually not *i* but assimilated to the vowel of the preceding word. Further, this might imply writing *tí* in case 3*a*, where the second vowel stands for a syllable originally beginning with a consonant. Therefore the second method, the apostrophe, should be adopted for all the cases of no. 3.

This would still leave us with the problem of the indication of the tone-glides on short syllables, both of the glides towards and from this elided syllable and of other junctional glides, such as, e.g., the realisation of a low tone after a high tone, which is a glide from the high tone downwards. I would suggest to leave these unindicated. They can be summed up in a fairly limited set of rules, which the foreigner should learn to observe, just as he has to learn when to pronounce English *l* as clear *l* and when as dark – distinctions which native speakers will introduce automatically.

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27) (Ad p. 64). R. C. Abraham, whose *Dictionary of Modern Yoruba* (London 1958) was published after the present paper went to press, draws attention to the interesting feature that such homophonic constructions with verbs in *-o* (ó g g^o o, a) 'it tires you', b) 'it tires him') are distinguished by the fact that in a) the high-mid glide is interrupted by a "diminution of breath" (Klingenheben's "consonantal vowel", BS), whereas in b) it is an "uninterrupted sweep" (p. XII). This might indicate an originally *consonantal* beginning of *o* in a): *o* < *-wo* < *iw*o as contrasted with the originally *vocalic* beginning of *o* in b): *o* < *-un* < *o*un — which would corroborate this interpretation of the 3rd pers. obj. pronoun and bring both forms in line with the other object pronouns, *mi*, *wa*, *yi*n, *wo*n.

ETYMOLOGISCHE BETRACHTUNGEN ZU NE. 'HEAL, WHOLE, HOLY'

Zusammenfassung

In neuerer Zeit wird in der sprachwissenschaftlichen Forschung die Verbindung zwischen diachronischer und synchronischer Betrachtungsweise, die sich schon vor mehr als zwei Dezennien anbahnte, betont. Die Untersuchung über den semantischen Komplex um ne. *heal, whole, holy*, etc. soll ein Beispiel für die moderne „Linguistique structurale“ (J. Fourquet), für das „Ineinandergreifen von deskriptiver und historischer Sprachwissenschaft“ (W. von Wartburg) sein.

Aus methodischen Gründen werden die sich an indoeurop. **koil-o-s* anlehnenden Bedeutungsnuancen in eine Sphäre der Integrität und eine Sphäre der Totalität (Intensität) geschieden. Von kulturhistorischem Interesse sind die aufgezeigten Parallelen zwischen germanischen und slavischen Begrüßungsformeln.

Summary

In recent linguistic expositions the combined method of both diachronic and synchronic investigation, which was initiated more than two decades ago, has been stressed. The article about the semantic complex of ModE. *heal, whole, holy*, etc. is to be regarded as an example in the line of the modern „Linguistique structurale“ (J. Fourquet), demonstrating the inter-linkage of descriptive and historical linguistics (W. von Wartburg).

For reasons of illustration and simplicity all semantic variations referring to Indoeurop. **koil-o-s* have been divided into a sphere of integrity and one of totality (intensity). The parallels between Germanic and Slavonic forms of salutation are, perhaps, not without interest for the historian.

“ ‘What is *heal*?’ asked Teresa.

‘It is to make *whole*’, said Dr. Glenister. ‘Our Lord touched the lepers and blind men and made them whole; sometimes He gives it to us to do this, Teresa; to make something live again . . .’ ”

Dieses Gespräch zwischen einem kleinen Mädchen und einer Missionsärztin in Kaschmir schildert Rumer Godden in “Kingfishers Catch Fire” (London 1953, p. 80). In geschickter Weise wird hier die etymologische Verwandtschaft zwischen *heal* und *whole* aufgegriffen, um die Bedeutung des ersteren („heilen“ = „ganz bzw. gesund machen“) zu veranschaulichen. Sogar die religiöse Bedeutungsnuance des „Heiligen, Geweihten“ oder eventuell auch im Sinne des „Heilers“ (ahd. *heilāri*;

vgl. ne. *healer*) oder „Heilenden“ (ae. *haelend*; ahd. *heilant*, nhd. „Heiland“) schimmert durch die erklärenden Worte von Dr. Glenister hindurch, indem auf den „Herrn“ = (*Holy*) *Lord* verwiesen und damit diese gedankliche Assoziation unwillkürlich hervorgerufen wird.

Meines Erachtens bietet der semantische Komplex (L. Bloomfield, „Language“ 1955, würde vielleicht von einem „sememe“ sprechen) um engl. *hale*, *hail*, *hallow*, *Hallowe'en* (Vorabend zu *Allhallows* = „Allerheiligen“; in Schottl. u. USA.), *heal*, *health*, *healthy*, *holiday*, *holiness*, *holy*, *wassail*, *whole*, *wholly*, *wholesome* sowie ae. *hālettan* = „begrüßen“ (ahd. *heilazen*; abulg. *cělovati*), *halsian* = „beschwören“, *hāel* = „Heil, günstiges Vorzeichen“, *hāelo* = „Heil, Seligkeit, Glück“ recht wertvolle und interessante Aufschlüsse für Sprach- und Kulturhistoriker.

Derselbe Komplex tritt im Deutschen mit den Wörtern „Heil, heil, heilen, Heiland, heilig, etc.“ auf, während dem engl. *whole* = „heil, ganz“ die Formen *hel* im Dän., Schwed. und *heel* im Holländ. (vgl. *heel* im Ndd.) entsprechen. Fügen wir dem noch die russ. Bedeutungsgruppe *celyj* = „unversehrt, heil, ganz, unverletzt, vollständig“, *celomudrennostj* = „Keuschheit, Unberührtheit“, sowie *celovatj* = „küssen“ (urspr. = „begrüßen“) hinzu, so läßt sich ohne weiteres auf die Urbedeutung einer gemeinsamen indoeurop. Wurzel**koil-o-s* = „unverletzt, ganz, gesund, heil, unberührt, keusch, unantastbar, immun, erhaben, heilig, etc.“ schließen.

Zur Urverwandtschaft von got. *hails* und altpreuß. *kails* vgl. Specht, S. 17; keltische und griechische Formen finden sich bei Brugmann, S. 42. Die etymologisch-semasiologischen Untersuchungen von Brugmann und Specht lassen in überzeugender Weise eine gemein-indoeuropäische Verwandtschaft erkennen. Hirts und Sandfelds Annahmen germanischer Lehnübersetzungen im Altslavischen entbehren m.E. exakter Beweise.

Als Dublette zur gesamten *whole*-Sippe sei kurz auf den synonymen romanischen Komplex um lat. *integer* (von *tangere* = „berühren“; vgl. *intactus* = „unberührt“), frz. *entier*, *entière*, span. *entero*, portug. *inteiro* (daher auch indones. *antero* = engl. *whole*), ital. *intiero*, engl. *entire*, *entirely* verwiesen.

Die oben erwähnten englischen Bedeutungsnuancen zu *hale*, *heal*, *whole*, *holy*, etc., deren semantische Grenzen infolge des unaufhörlichen Bedeutungswandels im Sprachleben durchaus fließend sind, könnte

man zu einem großen Semantem zusammenfassen, welches in der Hauptsache „Unverletztheit“ und „Ganzheit“ ausdrückt. Aus diesem Grunde erscheint für die vorliegende etymologische Untersuchung eine Gliederung in eine Bedeutungssphäre der „Integrität“ und eine Sphäre der „Totalität“ angebracht.

Sehr aufschlußreich für das Forschungsgebiet des Bedeutungswandels und der Funktionsverschiebung ist auch die Beobachtung, daß in vielen Fällen die Funktion der „Intensität“ aus der Bedeutung einer „Totalität“ hervorgegangen ist. So zum Beispiel bei den englischen Gradadverbien *all, altogether, completely, entirely, fully, perfectly, quite, totally, utterly, wholly*. Zur Funktionsverschiebung vom Adjektiv zum intensiven Gradadverb vgl. auch meine Studie über „Full, Right und Very. Ihre Entwicklung und Verwendung im Britischen und Amerikanischen Englisch“ (in: Wiss. Zschr. der Friedrich-Schiller-Universität Jena. Gesellschafts- und Sprachwissenschaftliche Reihe H.4/5, 1955/56).

I. INTEGRITÄTSSPHÄRE

Heutiges nhd. Subst. „Heil“ (= „Unversehrtheit, Gesundheit, Errettung, Erlösung“; lat. *salus*) sowie Vb. „heilen“ (= lat. *salvare*; got. *hailjan* = „wieder ganz bzw. gesund machen“) gehen auf dieselben Formen und Bedeutungen im Ahd. zurück. Doch neben dem ahd. Neutrum *heil* (ae. *hāel*) existierte noch ein Fem. *heilī* (ae. *hāelu, hāelo*. Vgl. die „Altenglische Grammatik“ von Sievers-Brunner, § 280). Sowohl *hāel* als auch *hāelo* treten im Beowulf unter der Bedeutung *health, welfare, luck* auf. Beispiele:

- a) „Grētte . . . Hrōdgār Beowulf, and him hāel ābead“ = „und entbot ihm Heil“ (Beow. 653; Holth.); „Brūc þisses beages, Beowulf leofa | hyse, mid hāele, ond þisses hrægles neot“ (Beow. 1216 f.).
- b) „Gesæet dā on naesse niðheard cyning | benden hāelo ābead heorðgeneatum“ (Beow. 2417 f.).

Demgegenüber gibt es aber auch *un-hāelo* = *mischief, destruction*, „Unheil“: „sorge ne cuðon, wonsceaft weras, wiht unhāelo“ (Beow. 119f.). Mit *wiht unhāelo* = „Dämon des Unheils“ ist Grendel gemeint.

In der Bedeutung „ganz, heil, unversehrt“ ist das Adj. *hāl* in Beow. 299 f., aufzufassen: „gōðfremmenda swylcum gifeþe biþ, | þæt þone hilderæes hāl gedigeð“. Ebenso Beow. 1974: „headolaces hāl“.

Die substantivische Bedeutungsverschiebung „Gesundheit > Glück > glückhaftes Vorzeichen“ (vgl. Beow. 204: „*hæel sceawedon*“) spiegelt sich auch im ae. Adjektiv *hæel*, *hāl* wider. In der oben erwähnten Grundbedeutung „unverletzt“ begegnet es z.B. auch im Poema Morale (Zupitza-Schipper, S. 89, Z. 114): „*hwa se segge þæt he beo hāl / him self wat betst his smeorte*“. Und so hat es sich auch noch bis ins Ne. erhalten, wo wir, besonders im Norden, die Form *hale* = „frisch, gesund“ neben dem üblichen *healthy* (von *health*) vorfinden: „*a hale man*“ (Gissing, „The Private Papers of Henry Ryecroft“. London 1903, p. X) = „ein rüstiger Mann“. Außerdem erscheint es häufig in der Alliteration *hale and hearty* = „gesund und munter“. Nach Jespersen („Growth and Structure of the English Language“. Oxford 1956, p. 62) ist die Form *hale* auf skandinavischen Einfluß zurückzuführen. „In some cases both forms survive in standard speech“, heißt es (a.a.O.) zu den skandinavisch-englischen Dubletten, „though, as a rule, they have developed slightly different meanings: *whole* (formerly *hool*) — *hale*; both were united in the old phrase ‘*hail and hool*’ “.

Sowohl die substantivische Bedeutung „Wohlsein, Glück“ (vgl. die ne. Interjektion „*Hail!*“ z.B. in *Hail Mary* = „Ave Maria“) als auch die adjektivische „gesund, wohl, froh, glücklich, wohlauf“ (vgl. „King Horn“; Zupitza-Schipper, S. 130, Z. 149 f.: „*and seie þe þaene kyng . . . þat ihc am hol and fer*“) wurde den Begrüßungsformeln in altgermanischer Zeit zugrunde gelegt. Vgl. folgende Belege aus alt- und mittelenglischen Texten: „*Wes þu, Hrōdgār, hāl!*“ (Beow. 406) = „Heil Hrodgar!“.

Dieser heidnisch-germanische Gruß wurde von alten Übersetzern ins Biblische übertragen. Es lohnt sich, die Bibelstelle Matth. 28,9 in ihren verschiedenen Übertragungsweisen zu vergleichen. Die Authorised Version (1611) bietet folgenden Wortlaut: „*And as they went to tell his disciples, behold, Jesus met them, saying, All hail*“. Wycliffe: „*Thesus ran agens hem seyinge: ‘heil ge’*“. Rushworth (Glossen): „*haelend quom heom ongaegn cwaepende beoþ hale*“. Nero D IV (vgl. Zupitza-Schipper, S. 64): „*haelend togaegnes arn ðaem cued wosað gie hal*“. Bodl. 441 und Hatton 38 (ibid., S. 65): „*hale wese ge*“. Vgl. hiermit die ahd. Tatianstelle (Braune-Helm, „Althochdeutsches Lesebuch“. Halle 1942, S. 43, Z. 4f.): „*Inti ingangenti thie engil zi iru quad: ‘heil uuis thû gebôno folli!’*“. Neben dieser analytischen Be-

grüßungsformel bildete sich schließlich auch ein Verbum ae. *hālettan* (ahd. *heilazen*, *heilezen*) = „beglückwünschen, begrüßen“ heraus. Ein ae. Beispiel hierfür findet sich in Bedas Bericht über Caedmon (Zupitza-Schipper, S. 48, Z. 26f.): „þa stōd him sum mon aet þurh swefn ond hine hālette ond grētte“.

Dazu vergleichsweise ein Beispiel aus dem Ahd. (Tatian, a.a.O., Z. 21f.): „*Maria . . . ingieng thō in hūs Zachariases inti heilizita Elisabeth. Uuad thō, sō siu gihōrta heilizunga Mariūn Helisabeth, gifah thaz kind in ira reve*“. Auch im Mhd. ist das Verbum *heilsen* = „Glück wünschen“ (zum Jahresanfang, bei Hochzeiten, etc.) üblich.

An dieser Stelle muß nun auch auf das russische Verbum *celovatj* = „küssen“ aufmerksam gemacht werden, weil dies doch eine in slawischen Ländern weitverbreitete Begrüßungsart (bei feierlichen Anlässen) ist und ursprünglich soviel bedeutete wie „Heil, Gesundheit, Glück, Segen wünschen“ (Adj. *celyj* = „ganz, unversehrt, ungeteilt“ entspricht völlig lautgerecht unserem „heil“ und dem englischen *whole*). Zu abulg. *cělovati* bemerkt Specht (S. 18): „Es heißt zwar ‘küssen’. Aber die alte Bedeutung ‘grüßen’ ist unter dem Einfluß der über Byzanz kommenden neuen Begrüßungsart aufgegeben worden“. Die altrussische Sitte, sich nach gemeinsam besuchter Ostermesse vor der Kirche unter freudigem Ausrufen der ritualen Worte „*Christos voskres!*“ („Christ ist auferstanden!“) zu umarmen und zu küssen hat Tolstoj zum Thema seines Romans „Die Auferstehung“ gewählt. Übrigens ist bereits im Kirchenslawischen das Verbum *cělovati* in der Bedeutung „begrüßen“ (vgl. oben die ae. und ahd. Beispiele) belegt. In diesem Zusammenhang sei auch auf die Aufsätze von Zubatý (Listy Filologické 28, S. 85), Specht (Kuhn's Zschr. 64), Hirt (PBB. 23, S. 332) und Sandfeld Jensen (Festschrift f. V. Thomsen, S. 169) verwiesen.

Ein Beispiel für ne. *hail* = „begrüßen, wertschätzen“ findet sich in der Rezension von Dr. S. Türck (Newcastle-upon-Tyne) über Graham Greene's „The Quiet American“, London 1955 (in: Mitteilungsblatt des ADNV. 2, 1957, S. 61): „*The reception by the critics was mixed; on the whole, they were impressed, some even hailing the book as 'his best since The Power and the Glory'*“. Das Wort *wassail* in dem „Yorkshire Wassail Song“ (um die Weihnachtszeit gesungen) ist meines Erachtens eines der schönsten Beispiele für Erhaltung ältesten Sprachgutes im modernen Englisch:

“We’ve been awhile a-wandering
 Amongst the leaves so green,
 But now we come a-wassailing,
 So plainly to be seen:

For it’s Christmas time,
 When we travel far and near;
 May God bless you and send you
 A happy New Year!”

Während es heute meist soviel bedeutet wie *to make merry at Christmas* (vgl. B. A. Young’s Gedicht „Wassail, Wassail” in *Punch* 11.12.57, 675), ist in dem zitierten Vers noch der ursprüngliche Sinn als einer *form of salutation* zu erkennen, denn *wassail* stellt nichts anderes dar als eine kontrahierte Form des ae. Grußes *wes* (*þu*) *hāl*!

Wie aus einigen der oben angeführten ae. Beispiele ersichtlich ist, wurde schon recht früh für „Jesus” (*Salvator*) das Wort *hælend* (as. *heliand*, ahd. *heilant* = Part. Praes. „der Heilende”) gesetzt. Damit wird der ganze Komplex um *hāl*, *hāel*, *hælan* bereits der religiösen Sphäre genähert, und so erscheint denn auch daneben ein germ. Adjektiv für lat. *sanctus*: ae. *hālig* (z.B. Beow. 381: “*hālig god*” und 686: „*hālig dryhten*”), me. *haly*, *holi*, *holie*, ne. *holy*; ahd. *heilac*, *heileg* = „heilig, geweiht”. Bei Brunner („Die Englische Sprache” I, 1950, S. 37) heißt es hierzu: „In vielen Fällen hat man aber heimischen Wörtern einen neuen christlichen Sinn gegeben oder aus heidnischem Wortmaterial geschickte Übersetzungen der kirchlichen Ausdrücke gebildet. . . . Etliche englische Ausdrücke sind durch die englischen Missionare auch in Deutschland verbreitet worden, so trat wohl hd. *heilig* nach ae. *hālig* an Stelle des älteren *wih*; . . . deutsch *Heiland* mag dem engl. ‘*hæland*’ nachgebildet sein”. Über das Vordringen christlichen Wortgutes in ahd. Zeit, „das von Norden her, etwa Fulda, die angelsächsische Mission mit sich bringt” und die Rivalität mit althergebrachten oberdeutschen Ausdrücken vgl. Brinkmann, S. 120ff. „Letzter Erfolg und siegreiche Dauer bleiben in allen diesen Fällen den nördlichen Prägungen versagt; nur *heilag* und *geist* vermochten die oberdeutschen Entsprechungen *wih* und *âtum* niederzuringen” (a.a.O., S. 122; Brinkmann verweist hier auf Beiträge von Braune, PBB. 43; Ochs, PBB. 45; Meißner, Z.f.d.A. 66 u. 67 sowie Loewe, Z.f.d.A. 67). Ich nehme daher an, daß man die Bedeutungs-

schattierungen der *integritas*: „unversehrt, keusch, gesund, heil, froh, gerettet, erlöst, geweiht, gesegnet“ auch in den sakralen Begriffen „Heil, Heiland, heilig“ (man denke auch an die religiöse Aufgabe der *Salvation Army* = „Heils armee“) erkennen darf. Und auch ein „Heiliger“ bzw. eine „Heilige“ verdienen diesen Titel doch nur deshalb, weil sie unter ihren Mitmenschen „heilend“ wirken oder gewirkt haben. Dies geht jedenfalls klar aus den Worten hervor, die Gilles de Vannes, Freund Abelards, an Heloise richtet: “*That is not the reputation you have, Heloise. The common people, and they are the best judges, say you are a saint. They say that there is no beggar comes to the gate at Argenteuil but leaves it blessing you: that they bring you wailing children, and they are quiet in your arms: they are wounded, and your hands make whole*” (Helen Waddell, „Peter Abelard“, London 1943, p. 301). Man vergleiche darüberhinaus auch das von Katharina Herkenrath in der Zeitschrift „Muttersprache“ (H. 9, 1957, S. 344) angeführte Zitat: „Da werden wir also heil, wo wir es dem, der unseren Namen ruft, gegenüber ganz und ungeteilt aushalten“. Die von F. Kluge („Etymologisches Wörterbuch der deutschen Sprache“, 1910, s.v. „heilig“) vorgebrachten Zweifel („Die Bedeutungs-entwicklung von heilig aus dem Subst. Heil ist nicht ganz klar“; vgl. auch Pappmehl-Rüttenauer, S. 4: „Die eigentlich sprachwissenschaftlichen Fragen dagegen nach Herkunft, Wurzel und ursprünglicher Bedeutung des Wortes, die ja durchaus noch kontrovers sind, sind hier nicht von Belang“) wären m.E. somit gegenstandslos. Es erscheint auch im Me. *hoolnesse* in der Bedeutung *integrity* (Skeat) = „Unbescholtenheit“ bei Chaucer (Boethius, Book IV: Prose VI, 201f.): “*Whether men liven now in swich hoolnesse of thoght*”; und neben dem sakralen *haliday* wie z.B. in “*Than fil it thus, that to the parish-chirche, / Cristes owne werkes for to wirche, / This gode wyf wente on an haliday*” (C.T., *Miller's Tale* 3307ff.) wird auch das verweltlichte „Feier-(Ferien) tage“ verwendet (C.T., *Reves Tale* 3952): “*On halydayes bi foren hir wolde he go*”. Ne. *to hallow* = „weißen, heiligen“ erscheint als me. *halwen* bei Chaucer in den Versen C.T., *Persones Tale* 919: “*And for to halwen mariage . . .*”, ebenso wie das ne. Subst. *hallows* als me. *halwes* in Chaucer's “*Book of the Duchesse*” (831): “*By god, and by his halwes twelve*”. Neben ne. *unholy* (= “profan, rücksichtslos, leidig, elend”) wie in “*If you see a dog scampering across the grass in Germany, you may know for certain that it is the dog*

of some unholy foreigner" (J. K. Jerome, "Three Men on the Bummel") könnte man auch das selbst-erniedrigende Part. Perf. *unhallowed* (= „unbefugt“) stellen in dem Satz aus Dickens' "Christmas Carol": "But the wisdom of our ancestors is in the simile; and my unhallowed hands shall not disturb it, or the country's done for".

II. TOTALITÄTSSPHÄRE

Während ne. *wholesome* = „heilsam, gesundheitsfördernd, nahrhaft“ neben allen anderen einleitend erwähnten Begriffen wie *hale*, *hail*, *hallow*, *heal*, *health*, *holy*, etc. in die Bedeutungssphäre der Integrität, des „Unversehrten, Gesunden, Unbescholtenen, Keuschen, Heilsamen, Heiligen“ zu stellen ist, gehören das Adj. *whole* und Adv. *wholly* mehr in das Gebiet der Totalität. Ich möchte jedoch betonen, daß im allgemeinen solche Gliederungen völlig unorganisch, weil dem dynamischen Sprachcharakter zuwider, und nur aus methodischen Gründen für die Darbietung zulässig sind. Es wird also immer eine Reihe von semasiologischen Grenzfällen geben, wie z.B. in *to get off with a whole skin* = „mit heiler (ganzer) Haut davon kommen“ oder auch in der deutschen Doppelung „heil und ganz“ bzw. russ. *cel i nevredim* (eigtl. = „heil und unbeschadet“; vgl. auch engl. *whole and undamaged* in Punch 5.3.58, 313).

Sonst aber erscheint es, wie auch in den Substantiven *wholeness* und *wholesale* in der Bedeutung „ganz, vollständig“. Über die Ausdrücke für den Begriff der Totalität in den indoeuropäischen Sprachen vgl. die Arbeit von K. Brugmann (Leipzig 1894; bes. S. 41) und speziell für das Englische E. Sapir ("Totality". Baltimore 1930).

Aus der Bedeutungssphäre der Totalität ist das ne. Intensivum *wholly* = „völlig, ganz, sehr“ hervorgegangen. Es hat sich in dieser gradadverbialen Verwendung seit me. Zeit bis auf den heutigen Sprachgebrauch gehalten. Zum Gebrauch im Me. vgl. Heuer (S. 21f.) und Fettig (S. 118ff.). Für das Frne. Kühner (S. 104) und das Ne. Borst (S. 132) sowie Krüger (§ 1045). Ferner wäre noch auf die Angaben des NED. sowie auf die semasiologische Untersuchung von A. E. H. Swaen, "Entirely, wholly, largely, frankly" (in Archiv f.d. Studium d. neueren Sprachen, Neue Serie Bd. 34, 1916, S. 48ff.) zu verweisen, in der einige Beispiele zu verschiedenen affektbedingten ("emotional") Bedeutungsnuancen aufgeführt sind. Doch muß der Verfasser selbst

zugeben (S. 49): "*Necessarily my view is subjective, as indeed the gradual change of meaning in adverbs to which I shall be referring is largely a matter of subjectivity*".

Die Entwicklung, ausgehend von der Bedeutungssphäre der Integrität-Immunität-Religiosität (*hale, wholesome, holy*) über die der Totalität (*the whole day*) bis zu derjenigen der Intensität (Gradadverb *wholly*) hat in dem gleichbedeutenden *entirely* eine Parallelerscheinung. Zur Verdeutlichung des formalen, semantischen und funktionellen Übergangsstadiums möge folgendes Beispiel aus dem amerikanischen Englisch dienen: "*And to whom do I owe this belief in me, the beginning of a whole new life*" (Bessie Breuer, "Pigeons en Casserole"; in: Brickell, "O. Henry Memorial Award Prize Stories of 1943". New York 1943, p. 125). Eine solche Übergangsstufe des Adj. *whole* zum intensivierenden "*bobtailed adverb*" ist weder bei Borst, noch in dem Nachtrag von G. Kirchner ("Gradadverbien im heutigen Englisch". Halle 1955) verzeichnet. Andererseits muß zugegeben werden, das in dem betreffenden Beispiel die Totalitätsvorstellung (= „eines ganzen neuen Lebens“) noch ziemlich stark nachwirkt, während im folgenden schon mehr oder weniger ein Intensivadverb zu erkennen ist: "*Mr. P. W. . . . issued a statement last night declaring that the action against him was 'wholly unjustifiable'*" " (Daily Worker, London, 19.1.52, 1).

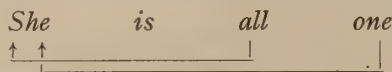
Besonders anschaulich läßt sich diese Verschiebung von modaler Bedeutung nach intensiver Funktion bei dem in die gleiche Bedeutungssphäre gehörenden *all* darstellen, wenn wir den Satz *She is all alone* einmal näher untersuchen. Gleichzeitig soll mit diesem Synonym zu *whole* gezeigt werden, „daß Bedeutungsentwicklungen auch bei etymologischer Unverwandtschaft in derselben Richtung verlaufen" (Brugmann, S. 3).

Bereits in *alone* aus me. *al-one* (ae. *eall-āna*. Vgl. auch mhd. *al-eine*¹) liegt eine ursprüngliche Verstärkung durch *all* vor. Andere Komposita, bei denen *all* als erstes Glied zur Gradbezeichnung des zweiten dient, sind im Ne. z.B.: *almost, almighty, although, also, already, altogether*, etc.

In dem Satz *she is alone* (= *all one*) könnte man *all* zunächst als

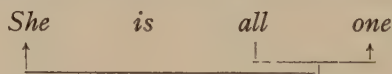
¹) Gelegentlich in den Frauenstrophen der Minnedichtung. Eigentlich genügt schon *eine* = allein, wie in den bekannten Versen "*Släfst du friedel ziere*", wo es in der zehnten Zeile heißt: "*du rîst und lāst mich eine*". Allmählich aber wurde *eine* für zu schwach empfunden und so verstärkte man es durch präfigiertes *al(l)*.

ursprüngliche Totalitätsbezeichnung zu *she* auffassen, etwa nach folgendem Bezugsschema:



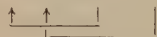
Vgl. dt. „einen ganzen langen Tag“; dial. „*a ganzer braver Bursch*“ > „*ganz a braver Bursch*“ und „*en jänzer juter Mann*“ (bei Krüger, § 489). Vgl. außerdem im Lat. flektiertes *totus* in ähnlichen Fügungen; ferner frz. *toutes pures*; ital. *tutta livida* und nhd. „eine rechte gute Frau“ (Lessing) ¹.

Hieraus konnte sich dann mit veränderter Funktionsrichtung (von *all* nach *one*) das intensivierende *all* entwickeln:

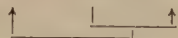


Genau so verhält es sich mit dem bedeutungsgleichen Intensivum *vesj* = „ganz, vollständig“ im Russischen:

A: *sachar vesj vyšel* (postattributiv) wird zu



B: *sachar vesj vyšel* (adverbial-intensiv) = „der Zucker ist völlig



aufgebraucht“.

W. Havers („Handbuch der erklärenden Syntax“, 1931, S. 76) führt solche Formen wie ndt. *n ganse ole fro* = „eine ganz alte Frau“, *n temlichen groten jung* = „ein ziemlich großer Junge“ oder auch frz. *toutes pures* darauf zurück, daß im motorischen Denkzentrum eine Artikulationsbewegung schon vorbereitet wird, „ehe sie, wie Wundt sagt, ‘eigentlich an der Reihe ist’“ (a.a.O. 74). Bei den eben erwähnten Beispielen handele es sich demnach um „Vorklang der Deklinationsendungen“. Es erscheint mir jedoch zweifelhaft, ob man solche Fügungen überhaupt als agrammatische „Vorwirkungsfehler“ betrachten muß, zumal sie auch Havers selbst an anderer Stelle als gebräuchliche Spracherscheinungen dem „Streben nach Anschaulichkeit“ zuschreibt; vgl. a.a.O., S. 149: „hier nur ein kurzer Hinweis auf

¹) Die letzten drei Beispiele sind bei Paul, „Prinzipien der Sprachgeschichte“, Halle 1920, S. 366 angeführt.

die malerische Wirkung des frz. Typus *elle est toute pâle*, wo das flektierte *toute* „die Erfassung der ganzen Persönlichkeit durch die Blässe“ ausdrückt, „sie ist in ihrer Gänze blaß“ (L. Spitzer). Damit vergleicht sich eine Stelle wie Ganghofer Ges. Schr. III S. 154: *Und sag mir nur grad, Blasi, was ist denn passiert? Du tropfst ja als ein Ganzer!*“

Der Verschiebungsprozess in dem oben zu *all* erwähnten Beispiel (und das ist das Interessante an ihm) braucht aber damit noch lange nicht aufzuhören. Das aus *all* + *one* verschmolzene *alone* wirkt mit der Häufigkeit des Gebrauchs nicht mehr emphatisch genug und kann daher von neuem durch *all* aufgefrischt und verstärkt werden. Der ganze Vorgang wiederholt sich und über die gleiche Entwicklung, wie oben veranschaulicht, ist dann folgender Satz zu verstehen:



Und sollte selbst dies für einen modernen Sprecher noch immer nicht kräftig genug wirken, dann kann er je nach individueller Affektveranlagung ein weiteres Gradadverb zu Hilfe nehmen, etwa: *She is absolutely all alone*. Dieser Satz ist natürlich wegen seiner krassen Hyperbolie ungewöhnlich, doch fand ich z.B. bei Lyell („Slang, Phrase and Idiom in Colloquial English and Their Use“. Kanda-Tokyo 1931) auch den Satz: *I am absolutely all in* = „völlig erschöpft“. Allerdings muß man andererseits bei der Suche nach treffenden Beispielen sehr auf der Hut sein, damit man sich nicht durch erste Entdeckerfreude in die Irre führen läßt. So stieß ich beispielsweise im *Punch* (16.11.55, S. 576) auf die Wendung „*All ready, Mr. Querdly*“ (im Sinne von *all set*) und bei Angus Wilson („*Hemlock and After*“. Penguin 1956, S. 123) auf die Verbindung „*All already, dear, that's what she said*“, wobei die Parallele zur Entwicklungslinie *all one* — *alone* — *all alone* völlig auf der Hand zu liegen scheint. Doch weit gefehlt: Wie aus dem Kontext hervorgeht, handelt es sich im letzten Beispiel um pronominales *all*. Man muß hierzu die vorausgehenden Sätze mit in Betracht ziehen: (ib.) „*I expect Father's worrying about it all*“ und „*though he did know it all already*“. H. Brinkmann (S. 27, Anm. 1) hat daher ganz recht, wenn er die Sprachforscher ermahnt, bei ihren Materialsammlungen mit „Satzzetteln“ statt mit „Wortzetteln“ zu arbeiten, denn, „Syntaktische

Forschung ohne Interpretation aus dem Zusammenhang heraus ist nicht möglich".

Abschließend sei noch einmal darauf hingewiesen, daß ne. *wholly* denselben Weg wie sein etymologisch unverwandtes Synonym *all* gelaufen ist, und sich infolgedessen bei der Fügung *a whole new life* der Verschiebungsakt „Totalität“ → „Intensität“ in derselben Weise wie bei *all* darstellen ließe. In Beispielen mit Adv. *wholly* wie in *a wholly good man* und *to dance wholly well* (Sapir, S. 25) ist offensichtlich die Begriffsnuance der Totalität zugunsten einer emphatischen Funktion gewichen, während sie beispielsweise in "*the decisions may not be wholly just*" (Brickell, a.a.O., "Introduction", p. 17) noch mehr oder weniger durchschimmert.

Jena

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A CASE OF COEXISTENCE OF PHONEMIC SYSTEMS *)

Summary

The pattern of consonants in the phonemic system of Friulan shows a peculiar arrangement, according to the phonetic evolution going on in different local varieties.

In the present paper are examined the phonological problems inherent in this situation, and a possible graphical solution is suggested.

The problem of coexistence of phonemic systems has been dealt with many times ¹⁾, but always with stress on the questions of acculturation, bilingualism and loan. The common starting point in dealing with this problem is the interference of foreign phonemes with a system already established. General textbooks and papers on phonemics stress the fact that foreign words in the context of another language are anything but infrequent. However, they do not generally consider such words as a problem to be solved within the limits of the given system; rather, according to circumstances which cannot be gone into in this paper, they may or may not be interpreted as belonging to a different system. When different languages come into closer contact, two effects on the speakers can be seen: they may either adopt the new sounds and phonemes without attempting to incorporate them into the system, or they may try to fit them into the system, with or without requiring modifications of the loan words and of the system itself.

For the linguist, on the other hand, when he considers a case of the adoption of new phonemes, there are two possible solutions ²⁾: either the corpus is considered as a closed whole, all of which is to be given the same analytic treatment; or the foreign elements ³⁾ will be disregarded. The first solution will lead to quite different definitions and

*) I am very indebted to dr. L. Newmark, of Ohio State University, for most valuable help in revising my english style, and many linguistic suggestions.

¹⁾ See, e.g., Fries and Pike, *Coexistent phonemic Systems*, Language, XXV, 1949, and W. L. Wonderly, *Phonemic acculturation in Zoque*, IJAL, 12, 192—95 (1946). For some theoretical views of the problem, somehow related to the ones expressed in the present paper, see U. Weinreich, *Is a structural dialectology possible*, Word, 10. 2-3, 1954, pp. 388-400.

²⁾ See Z. Harris, *Methods in Structural Linguistics*, Chicago, 1951, pg. 9—12.

³⁾ This solution is generally only adopted if they belong to a well known language.

a different inventory of the phonemes, but it corresponds very closely to the point of view of the unsophisticated speaker. The second solution helps to maintain the system in its original form, and may well serve purposes of comparison. Both solutions have advantages as well as disadvantages, either of which may be taken into consideration for particular reasons of convenience.

A discussion of complete integration into the language (and acculturation into the culture) would probably lead us too far, e.g. to ask whether *g^w* is to be taken as a foreign segment in Italian in examples such as *guerra* (< germ.**wer*) since it reintroduces into the system a type *g^w* long lost in Latin. To put it that way, loan words are loan words as long as they can be actually recognized as such by the speaker without help of philological technique ⁴).

In contrast with these solutions, which view the problem of bilingualism and language contact as a clash between separate languages ⁵), the problem of coexistence of phonemic systems can gain further insight into the life of language by being studied in a closely related field, that of dialectology. In general such problems more frequently are felt to be important by European linguists; they seem to have a broader understanding and a deeper concern with them exactly because most linguists in Europe have been able to experience the facts themselves. In many cases the European linguist in his boyhood was forcibly (and not without pain) obliged to make use, very early in his life, of some sort of literary or national language, to which was attached the full light of social prestige, while his mother speech, the dialect, had to remain in the shadow. Still, the charge of psychical and emotional expressivity, which adheres to the use of dialect, will for him suddenly turn up at the simple sound of the native 'patois' of his birthplace. In this sense, one can affirm that in Europe almost everyone is a bilingual, and has continuously to fight against elements from his native speech, to force them back again into the shadow of half consciousness, of disregard or even contempt, when he contrives to speak on a 'higher' level.

For purposes of phonemic analysis, dialect distinctions are intro-

⁴) Of course, this offers only a subjective criterion.

⁵) This problem is the subject of the book by U. Weinreich, *Languages in contact*, New York, 1953 (but see the writer's review in *Archivio Glottologico Italiano*, 74—77, 1956).

duced only as a simpler way to state the facts as they are. In the introduction to the analysis a statement can be made about what has to be considered 'one dialect' and the variations can be taken into account by a marking device ⁶⁾.

Now it may well happen that a language splitting into several dialects can be easily dealt with by such a simple marking procedure. There are, however, cases in which we can neither assume that the language is uniform for the entire group of speakers, nor easily recognize fixed different dialect sections, with different phonemicization. Of course, to the layman the problem is of no relevance, since what he perceives is clearly a single language, where only minor phonetic discrepancies are to be heard. But to the descriptive linguist it will appear rather unsafe to state that one 'Langue' has two different phonemic systems; the obvious answer for him will be that there are two different dialects.

Of course, we will always accept the last formulation, when we see that the segments appearing in one dialect have regularly corresponding variants in another dialect. Elsewhere we will readily accept as different dialects such cases where we find new phonemes introduced in one dialect but not in another. However, there may be some cases, which make it difficult to state definitely that we are dealing with two different phonemic systems.

One such case is Friulian, a known romance language of northern Italy, whose historical and geographical outlines are easy to gather from much published linguistic material, and quite recently from a paper published in WORD ⁷⁾. In this paper, under the title "Friulian Phonology", a phonemic system of the language is given: however, it is explicitly stated that this system belongs to the variety of Friulian as spoken in Udine (the main city of the region). According to the analysis given in the paper, the consonant system of Udinese-friulian comprises the following 17 consonants:

<i>p</i>	<i>t</i>	<i>k</i>	<i>f</i>	<i>s</i>	<i>č</i>		
<i>b</i>	<i>d</i>	<i>g</i>	<i>v</i>	<i>z</i>	<i>ǵ</i>	<i>l</i>	<i>r</i>
<i>m</i>	<i>n</i>				<i>n</i>		

⁶⁾ As Z. Harris suggests (*Methods*, pg. 11).

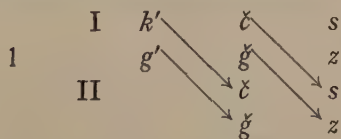
⁷⁾ B. Bender, G. Francescato, Z. Salzman, *Friulian Phonology*, WORD, VIII, 3, 1952, pg. 216—223.

We can disregard here the demonstration of the phonemic value of these consonants, since it is to be found in the quoted paper. We will rather direct our attention to the set of 4 palatal and spirant consonants $\check{c}$ $\check{g}$ s z . The phonetic realization of these 4 consonants can be described as follows:

$\check{c}$ $\check{g}$ palatal affricate
 s z spirant
 voiceless and voiced respectively.

Most romance scholars will notice that this set seems to lack two phonetic values, viz. the peculiar pre-palatal sounds (the voiceless and the voiced one) which are well known to students of Rhäetoromanic languages (as Friulian) and are supposed to form a phonetic characteristic of the family. We will write them phonetically k' and g' and we will accordingly be tempted to look for them as phonetic realizations of the above phonemes in some other variety of Friulian, since they do not appear as allophones in Udinese-friulian.

Now, we have shown in a previous paper ⁸⁾ that these two sounds are really lacking in Udinese-friulian, as well as in many other Friulian varieties, while they still survive and are to be heard in everyday speech in many other varieties. As far as the geographical distribution of the varieties is concerned, the reader is referred to the paper, where the dialectological data are fully displayed. In that paper, however, we not only demonstrated that k' and g' exist, and how they are distributed, but incidentally point out that they cannot be considered as simple substituting variants for the phonetic realizations of the phonemes of Udinese-friulian. Rather, we can contrast two different phonemic systems, one of which is to be attributed to one variety of Friulian, and the other to another variety. If we juxtapose the prepalatal-palatal-spirant set of these two varieties (symbolised I and II respectively) the pattern would look as follows:



⁸⁾ To appear, with the title *Consonanti prepalatali e palatali in Friulano* in the *Atti dell'Istituto Veneto di Scienze, Lettere ed Arti* (Venice).

The historical evolution represented here is still going on today in the directions shown by the arrows, so that pattern I has to be considered as the original one. We suppose that, following the shift $\check{c} > s$ and $\check{g} > z$ we had an evolution $k' > \check{c}$ and $g' > \check{g}$. Both processes occur in a group of innovating Friulian varieties (pattern II), but in some cases the first only can be found. In the conservative varieties on the contrary, pattern I is still maintained. Moreover, in a few innovating varieties k' and g' will be revived on a different etymological basis.

The phonemic status of the two questioned segments k' and g' can be proved by setting up a tabulation of the regular replacements $g' > \check{g}$, $k' > c$ and $\check{g} > z$, $\check{c} > s$ in their various environments. In the following chart a + indicates occurrences of the segments in the environments symbolised above, V stands for vowel, # = for word juncture. The chart proves only the phonemic contrast of the prepalatal against the palatal series, and of the palatal against the spirant. The contrast between prepalatal and spirant in pattern I can be shown by such examples as *fik'à* 'to drive in' / *fisà* 'to fasten'; *vìg'e* 'Louise' / *vìze* 'inform (v.)' (where no historical processes as the one in discussion are involved).

	# — V		n — V		V — e		r — e		V — o		l — V		V — i		r — i	
	I	II	I	II	I	II	I	II	I	II	I	II	I	II	I	II
g'	+		+		+		+		+							
$\check{g}$	+	+	+	+	+	+	+	+		+			+		+	
z		+		+		+		+			+			+		+
	# — V		^a i — V		^e o — V		r — V		^V s — #		^V n — #		^r l — #		V — #	
k'	+				+		+		+		+				+	
c	+	+	+		+	+	+	+		+		+		+	+	+
s		+		+		+		+					+			+

It can be seen that in many environments (four for the voiced and four for the voiceless segments) the segments will have a contrasting value in all dialects. So, for instance, in conservative varieties we can have contrasts such as: *k'ère* 'earth' / *čère* 'wax' and in innovating varieties contrasts such as: *mànġe* 'he eats' / *mànze* 'cow'. In some cases, an entire set of double contrasting pairs can be found: *mànġ'e* / *mànġe* vs. *mànġe* / *mànze* (in the double pair *k'ère* / *čère* vs. *tière* / *sère*

one element is lacking, because in most innovating dialects the group *ti* has not developed into *č*).

As we have said, there are also a few innovating areas, where *k'* and *g'* have phonetically been revived, because of a phonetic change from *-tiV-* to *-k'V-* (and also sporadically *-diV-* to *-g'V-*). In this case we have a triple contrast in innovating as in conservative varieties:

<i>k'</i>	<i>g'</i>
<i>č s</i>	<i>ğ z</i>

which appears in series such as: *k'ère* 'earth' / *čère* 'wax' / *sère* 'evening'.

Based on this evidence, a very reasonable statement of the overall Friulian pattern would seem to require the three series of phonemes:

<i>/k' č s/</i>
<i>/g' ğ z/</i>

The only other solution would be to try a rephonemacization, namely to analyse each of the segments *k'* and *g'*, whose distribution is highly restricted, as sequences of two already established phonemes in distributional patterns found for other analogous sequences⁹⁾. Of course, we disregard a priori such sequences which would lead again to irregularities in the system. Given this provision, the only possibility we have left is to solve */k'/* and */g'/* as either the sequences */ti/*, */di/* or as */ki/*, */gi/* respectively. This solution is supported by the occurrence of such other sequences as */pi, bi, ti, di/* (e.g. *piés, biât, tiàre, diàul*). However, while it could be possible to accept the solution for all the environments of *## — V* or *X — V* (where *X* means any vowel or consonant), we cannot account in that way for the very few final contrasts (environment *V — ##* or *C — ##*). We would have in fact for */ti/*: [*dùk'*] 'all' = */dùti/* contrasting with [*spièti*] 'I wait' = */spièti/*, and [*tànk'*] 'many' = */tànti/* contrasting with [*k'ànti*] 'I sing' = */tiànti/*, or else [*imprèsk'*] 'tools' = */imprèsti/* contrasting with [*rèsti*] 'I remain' = */rèsti/*. For */ki/* we would have at least one contrast in environment *V — ##*, that is [*dùk'*] 'all' = */dùki/* contrasting with [*lèki*] 'I lick' =

⁹⁾ We did not consider from the beginning the fact that *k', g'* could have been interpreted as groups of two segments, since they show minimal contrast with *č, ğ* respectively, and these have been already interpreted as one-segment phonemes (see the discussion in the above quoted paper *Friulian Phonology*). They can be now interpreted as clusters.

/lèki/. For the voiced segment namely *g'*, the corresponding analysis into /di/ or /gi/ would lead us to write sequences such as /màngii/ or /màndii/ = [màng'i] 'I eat'. While it is true that in Friulian sequences of two vowels are not lacking (e.g. /famèe/ 'family', /làa càze/ 'to go home') we must recognize that such sequences occur over a morphemic or syllabic boundary; this is not the case for /—ii/ of /màndii, màngii/.

We are therefore obliged to set up in our 'overall' system two more phonemes, of restricted occurrence, that is /*k'*/ and /*g'*/: but we have to recognize that these phonemes never occur¹⁰) in the speech of certain varieties of Friulian. In spite of such local variations, the speakers of one variety of Friulian understand the other varieties as well, and are aware of the inherent unity of the Friulian language as a whole. Rather, a speaker of the *k'*, *g'*- varieties hearing somebody else speaking without making use of the two sounds, would tend to classify the other as a native of certain parts of Friuli. The same applies to the speaker of the *k'*, *g'*-less varieties: a speech containing these two sounds implies for him nothing more than the possibility of guessing with more or less precision the area from where the user of that speech comes. Different varieties may enjoy more or less prestige, according to particular points of view. But the fact that the innovation moves along, reaching for new geographical areas, and affecting them phonetically as shown by the arrows (see above, table 1) is a 'dynamic' reality, on the basis of which we can foresee that in a few years the whole language will be leveled to a prepalatal-less system. In such a system, the distribution of /*s*/ and /*z*/ will be obviously increased to include the possible environments of /*č*/ and /*ǵ*/, while /*č*/ and /*ǵ*/ will include the present distribution of /*k'*/ and /*g'*/ . At the present stage of development we have to accept the fact that the speakers of the prepalatal-less varieties find the two series fully adequate for using the language, and for them, so to speak, it contains only potentially the phonemes /*k'*/ and /*g'*/, whose contrastive function they will never fail to understand, but will also never use.

We can now try to make use of the marking device we have already mentioned. We might agree to mark with an initial * all the utterances of the conservative group of dialects, as opposed to the innovating

¹⁰) To say *never* does, of course, not rule out possible sporadic occurrence: it refers to the norm.

ones, which are left unmarked: but our marking device would not work this way, since conservative dialects too have the contrasts $\check{c}/s$ and $\check{g}/z$. In other words, we could write phonemically $/*\check{c}\grave{a}se/ = [k'\grave{a}\check{c}e]$ 'ladle' but we could not write a word like $[k'\grave{a}ze]$ 'house'. Therefore we are obliged to mark not utterances, but phonemes; e.g. we could mark with a bar $\bar{c} = k'$ and $\bar{g} = g'$. The graphic economy reached by this device is not particularly significant, because it does not permit us to eliminate two segments¹¹): however, it does permit us to restate our system and the dynamics of it in a more convenient way.

In conclusion, if we agree to write the k' and g' of table 1, I (pg. 81) as $\bar{c}$ and $\bar{g}$ respectively, and $\check{c}$ and $\check{g}$ of table 1, I as $\bar{s}$ and $\bar{z}$ respectively, we obtain the following scheme, which will enable us to preserve graphically the identity of cognate units, as they correspond from one group of dialects to the other, the cognates now showing the only difference of the superimposed bar, to indicate dialect diversity:

Conservative	$\bar{c}$	$\bar{g}$	$\bar{s}$	$\bar{z}$	s	z
Innovating	$\check{c}$	$\check{g}$			s	z

We can write now all utterances according to the contrastive system proper to the innovating area, e.g. $[t\check{s}\grave{a}ze] = |\check{c}\grave{a}ze|$. A bar over the varying phoneme symbol¹²) will enable us to show that the utterance represents some conservative dialect, where $|\bar{c}\grave{a}ze| = [k'\grave{a}ze]$. We might even speak of a component of tongue position: in this case the bar is the sign for more fronting of the tongue.

Still, it cannot be denied that, in conservative areas, there exists a three step contrasting system, while contrasts in the innovating area are reduced to two: but the overlapping of the phonetic realizations, complete as it may be, does not prevent the speakers from thinking of the language as a single one.

The problem is not lacking in theoretical interest, since probably in

¹¹) We thus have no longer the symbols for two phonemes k' and g' but our inventory of phonemes is increased by the symbol $\bar{}$ to be used when writing utterances of the conservative dialects. So we do not fully attain the results aimed at by Z. Harris, *Methods*, chapter 10, where phonemic components are treated.

¹²) The term "diaphone" suggested by D. Jones (*The phoneme*, Cambridge, 1950, pp. 139 etc. (chapter XXVII)) might be useful to apply here, but we prefer to avoid the implications that Jones has attached to it.

many cases contrasts between dialects (or dialects vs. language) — such as we find so often in Europe — might suggest the concept of potential phonemic relevance to explain the fact of actual understanding by speakers who never make use of the given relevant difference¹³).

The Hague

GIUSEPPE FRANCESCATO

¹³) Something of this sort can be supposed to be at the bottom of the well known possibility, for some speakers of ancient greek, to switch easily from one dialect variety to the other.

REVIEWS — COMPTES-RENDUS

N. S. TRUBETZKOY, *Altkirchenslavische Grammatik. Schrift-, Laut- und Formensystem*. Herausgegeben von R. Jagoditsch. (Österreichische Akademie der Wissenschaften. Philosophisch-historische Klasse. Sitzungsberichte, 228. Band, 4. Abhandlung). Rudolf M. Rohrer, Wien 1954. 197 S.

The great linguist Prince N. S. Trubetzkoy died in 1938. His posthumous Old Church Slavic grammar, so long awaited, appeared therefore only sixteen years after his death, under the editorship of Professor Jagoditsch. In his foreword Trubetzkoy sums up the textbooks which had appeared since the first World War, and justifies his adding one more to this number by noting that all these works leave out of consideration general linguistics and modern linguistic theory. Professor Jagoditsch has completed this list up to 1952. Trubetzkoy's statement on the methodological procedure in all the existing grammars was still applicable at that time, as he remarks.

It is certainly regrettable that this book was published for the first time so long after it was written. Because the writer was one of the founders of phonology as a science independent of phonetics, one is inclined to regard all his pronouncements as inherent to the phonologic point of view. The opponents of phonology – who are still to be found among the linguists – have often enough used a part of a theory which is unacceptable to them as an argument against functional linguistics in general. Needless to state, this danger is great in the case of the book we have before us here, since the theories expressed, as well as being personal ones, have also in part been left behind by the times.

Trubetzkoy was conscious of the new contribution he made with this book. He predicts in his foreword the disappointment which his work will arouse in many slavists. Because he limits himself to what "mit streng rationalen Methoden den Quellen entnommen werden kann", "sieht die altkirchenslavische Lautlehre nicht nur ganz anders, sondern auch viel 'abstrakter' aus" (p. 11). One of his principal points of departure is: "Das Altkirchenslavische erscheint... als eine rein literarische, d.i. mehr oder weniger künstliche Sprache, und die 'Mund-

arten der Schreiber', die sich aus der Orthographie der Denkmäler ergeben, sind keine Mundarten im eigentlichen Sinne, sondern nur verschiedene, hauptsächlich durch lokale Schreiberschulen zugelassene Abweichungen von der idealen Norm, wobei diese Abweichungen gar nicht mit der Umgangssprache zusammenzuhängen brauchen" (pp. 8-9).

The two most spectacular points in phonetics in which Trubetzkoy's ideas, as expressed in this book, differ from those previously current, are his interpretation as *ü* and *öN* of the vowels which in previous grammars had been considered *ju* and *jö*, and the representation of the nasal vowels as *oN*, *eN* and *öN*, thus, as groups of two phonemes. This departure from custom is all the more striking, since it gives an unusual appearance to the many examples cited in the work. In this respect one can have only praise for Trubetzkoy's consistency. "Ein Bruch mit der Tradition ist immer schwierig", he says himself. We may add that he never shrinks from this difficulty.

The author has made an effort to describe the oldest stage of literary Church Slavic, that is, the Church Slavic of the times and the milieu in which the alphabet was created. His conclusions as to the inventory of phonemes are drawn on the basis of a careful comparison of the Greek and the Old Slavic graphic systems, in which he takes as a point of departure that the Slavic (glagolitic) alphabet was made by a man whose graphic imagination ("Schriftdenken") was that of an educated Greek.

Of the two striking phonologic interpretations mentioned above one, the first (*ü* in place of *ju*) may be accepted. There are various indications that *j* in Old Slavic was not a separate phoneme. The strongest argument is still perhaps the fact, supposed by Durnovo, that the glagolitic character for *ü* was a borrowing from the Greek graphic cluster *oi*, which indeed was used for *ü*. In the dialect which served as a basis for the alphabet, the *j* existed only phonetically: the phonemes *e*, *i*, *ä*, *ü*, *ö* (the last only in the group *öN*) could be realized phonetically as *je*, *ji*, *jä*, *jü*, *jöN*. In addition Trubetzkoy assumes, that there were at the time of the first Bulgarian empire dialects which in initial position and after labials recognized an opposition *je* vs. *e* and at the same time, in the latter position, an opposition *jä* vs. *ä* (e.g. *zemjä* vs. *mädi*, p. 41).

In regard to the acceptance of a separate phoneme *N*, we should be

sceptical. The glagolitic character which we usually pronounce as ϵ (eN), was read by Trubetzkoy as N (again, this only applies to the oldest period). One thus has groups of phonemes eN , oN , δN ; the last containing a phoneme δ which is never encountered in any other position. The existence of such a phoneme is in itself unlikely. Furthermore, it is far from impossible that the creator of the alphabet represented a phoneme ϕ by a combination of $o + \epsilon$ (and ϕ by a combination of a sign δ to be found nowhere else with ϵ). This would indeed be comparable with the representation of u by $o + \ddot{u}$, and by y by $u + i$, a supposition which was expressed by Vaillant, but which was briefly denied by Trubetzkoy (p. 24, n. 2) with the, in my opinion invalid, argument that such a ligature would only be used for a difference in position of the tongue (u vs. $\ddot{u}$, y vs. i), while in oN vs. eN lip-rounding plays a role as well. Apparently, Trubetzkoy here overestimates the phonetic insight of the creator of the alphabet, who was probably satisfied with the resemblance of ϕ and o .

In morphology Trubetzkoy makes a strict differentiation between productive and unproductive forms. This method of working gives us a better understanding of the systems of declension and conjugation than the historically oriented morphology is capable of.

On the other hand, the way Trubetzkoy works with "Verbindungsmorpheme", "die lediglich der Verbindung von anderen Morphemen dienen", seems to us too mechanistic: this is a phenomenon from the infancy of structural linguistics which we have now outgrown. Trubetzkoy regards the $-e-$ of *nes-e-tŭ* as such a connective morpheme. So also the $-e-$ of *nes-e-te*, which is exactly the only formal sign by which this indicative can be distinguished from the imperative *nes-ä-te*. Also the $-äa-$ in *nes-äa-xŭ* appears to belong to this category, whereas it is evident that it is the formal sign of the tense (imperfect).

This posthumous work of Trubetzkoy is fascinating reading for anyone who is interested both in general linguistics and in Old Slavic, but, for the above reasons as well as because of the incomplete condition in which it has come down to us, one could hardly use it as a handbook.

HORACE G. LUNT, *Old Church Slavonic Grammar*. (Slavistic Printings and Reprintings, edited by C. H. van Schooneveld, III). Mouton & Co., Den Haag 1955. XIV + 143 pp.

If Trubetzkoy's Old Church Slavic grammar was the first attempt to present a scientific description of the Old Church Slavic language on modern principles, the work before us is the first textbook of this sort. In his review of Trubetzkoy's book (*Language* XXXI), Lunt discusses what, in his opinion, the point of departure should be in the composition of such a textbook: "I cannot agree that... P[roto]-C[hurch]-S[lavonic] is what new students should learn. The facts we have for analysis are primarily the facts of Trubetzkoy's Bulgarian O[ld] CS. Normalization is a pedagogical necessity for the initial stages of instruction, but the norm set up should be as close to the attested facts as possible. It is only after the student has learned this kind of norm and has seen, from the real texts, just what the idealized nature of this description is, that he can be taken one further step in abstraction to Proto-OCS. And this is already the realm of a course in comparative Slavic linguistics, which should not be confused with the study of individual languages".

This was undoubtedly the point of departure of the author himself in writing his grammar: the whole work bears witness to it. On one hand Lunt chooses a completely different standpoint to Trubetzkoy in regard to the stage of language to be described, and he continues in the great tradition which began with Leskien and in which Diels' enormous collection of material forms an apogee (Lunt brings in the nature of the case practically no new material and for details simply refers to Diels). On the other hand Lunt adheres as carefully as Trubetzkoy to the demands which modern linguistic science makes for the description of a language.

The search for a norm places the author in the difficulty which indeed he mentions in his introduction, namely that the manuscripts on which the description must be based differ a great deal from each other. But even greater than the differences between the phonologic systems represented in the manuscripts is the difference between these systems on the one hand and, on the other, the system of the milieu in which the glagolitic alphabet came into being. Therefore, the final description must leave room for play without placing the phonologic principles in jeopardy.

As an illustration of how the author acquitted himself of this task, let us cite the two tersely formulated paragraphs 2.03 and 2.04: "The alphabets indicate two vowels, *ü* and *ö*, and they do not have a yod, while comparative evidence points rather to a yod and the combinations *ju* and *jö*. Thus the spellings *üže* and *öže* could be interpreted either as *üže/öže* or *juže/jöže* [the latter form is a misprint for *jöže*]. (Of course, this does not necessarily mean any phonetic difference: the phonetic yod could be in one case merely a concomitant of the front vowel not after a consonant, and in the other an independent phoneme.)"

"Cyril's dialect had a single phoneme *ě*, where other dialects had an opposition of *ě* – *ja* ... (And again, it does not mean any striking difference in phonetics)".

The two above mentioned facts place Cyril's dialect in a special position, not only in respect to the dialects represented by the manuscripts, but also in respect to all modern Slavic languages. All Slavic languages have a phoneme *j* and not one of them knows a phoneme *ü*. When Lunt repeats the generally accepted opinion that Old Church Slavonic is "a form [of Slavic] very close to the language called Proto-Slavic or Common Slavic", this pronouncement is actually more applicable to the language of the manuscripts than to that of the Slavic apostles, although the former are chronologically younger than the latter. We might even, in the case of interpretations such as that of *ju* as *ü*, consider the influence of bilingualism. For this reason we regard Lunt's comment (also a commonplace), that "the two Slavic alphabets are admirably suited to the languages for which they are devised", as a half-truth. For the glagolitic alphabet it is indeed so, although we have no manuscript in this language for which it was devised (e.i. in which the letter *ü* does indeed represent "one letter for one significant sound"); the cyrillic alphabet, on the other hand, was probably intended for a language in which a phoneme *j* existed, and was thus, as an all too slavish copy of the glagolitic, entirely unsuited to reproduce this language. This is a very important failing on the part of this alphabet, which unfortunately remained with it during the greater part of its conquest of the Slavic lands (only the forms which are currently in use for Serbocroatian and Macedonian are free of this evil), and with which even some non-Slavic peoples in the Soviet-Union now have to struggle.

Lunt therefore, with full justification, is led to set up two phonologic systems, one for the language of Cyril and Methodius, the other a "more generalized system", which is represented in the manuscripts. A few questions remain unsolved in his description. For example, he says of the *j*, that "it may have been a non-syllabic variant of *i*, and not an independent phoneme". Here a certain discrepancy arises with his transcription of the clusters *lj*, *rj*, *nj*: the phonemes following the *n* are after all distinctively different in such words as *svinii* 'swine' and *elenji* 'deer's' (adj. nom. pl. masc.). Indeed Lunt says himself that these clusters most likely "were unit phonemes (palatals) in some OCS dialects", so that in these clusters we need see only a historical spelling. It becomes more difficult to regard the *j* as a combinatory variant of the *i* in the dialects in which forms like *zemji* (written *zemii* or *zemi*) 'land' (gen. sing.) occur.

In morphology the author, in accordance with the current tendencies, has aimed at a concise description of the facts. Where he deviates from this principle, he gives "pedagogical reasons" (cf. preface and p. 80, n. 8). In general this has as a result a great lucidity. Only here and there does it seem to us that the effort to attain regularity in the description must not be carried too far. In the case of the verb Lunt selects as "basic stem" either the infinitive without the ending *-ti*, or the third person plural of the present with omission of *-otŭ*, *-etŭ*. The longer of these two fragments is the "basic stem". The naming of some verbs by the one form and of others by the other, resulting in such lists as: *alkati*, *želějotŭ*, *židati*, etc., seems to us decidedly inelegant. Now and again this attempt at simplification leads to greater involvedness, because Lunt adheres too closely to this one "basic stem". So, in our opinion, just exactly for people who must learn the language, the old rule for finding the third person singular of the aorist remains in many cases preferable to Lunt's new method. Formerly one said: if, after dropping the infinitive ending *-ti*, a stem ending with a vowel remains, that is the form, but if the stem ends in a consonant, you must add an *-e*. Lunt derives first from the forms *dělajotŭ* and *delati* the "basic stem" *délaj-*, and gives the following rules (p. 87): "A final *-j* is lost and the aorist stem ends in a vowel"; (p. 88): The ending needed for the third person is *-e*, but "if the aorist stem ends in a vowel, the ... *e* of the ending is dropped". The terminology here is also unfortunate: By "is dropped" he apparently means "is lacking"

or perhaps "is not added". This is naturally a question of subordinate importance, but this discrepancy between the terminology and the principles followed might have been avoided without difficulty. In those cases, too, in which such historical terminology may really be understood as historical, without bringing us into conflict with the facts, we would rather, considering the character of the work, that they had been avoided, as on p. 46, where it is written: "Masculine nouns signifying male persons replace the form of the accusative singular by that of the genitive singular".

At the end the book contains "notes on syntax". It is praiseworthy of the author that he has not left this virgin terrain entirely untouched. That this chapter cannot be placed on the same level as other parts of the book is something for which one can hardly consider the author responsible. A textbook simply sums up the results of previous investigation. At the same time it here again becomes evident that the modern tendency in linguistics has not yet come so far as the syntax.

On the other hand, certain imbalances are to be encountered in the book, which can indeed be laid at the author's door. The meaning of the aspects is discussed under morphology (p. 69), that of the other verbal categories under syntax (pp. 135-137: *iskaaxo* is here incorrectly called an "imperfective aorist"). In the discussion of the cases (p. 126), the nominative is referred to as a "zero-case", while it is not said what in this respect is opposed to it in the other cases: of the accusative and the genitive only the syntactic usage is treated (pp. 126-129).

All these remarks refer to details and do not diminish our satisfaction over the publication of this book. It is indeed the herald of a renewal of instruction in Old Church Slavic. So far as its technical appearance is concerned, it might have been better: there are several misprints, those connected with the *e* and its diacritical marks sometimes misleading. We note only: p. 66, 1.4, read *e* instead of *ě*; p. 84, 1.10, read *ě* instead of *ę*; p. 89, 1.30, read *ę* instead of *ě*.

Amsterdam

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L. SADNIK, R. AITZETMÜLLER, *Handwörterbuch zu den Altkirchenslavischen Texten*. (Slavistic Printings and Reprintings, edited by C. H. van Schooneveld, VI) Mouton & Co., Den Haag 1955, (in Verbindung mit Carl Winter, Heidelberg). XX + 341 S.

This dictionary consists of three parts: the first contains an alphabetical list of all the words which occur in the Old Church Slavic manuscripts (with occasional grammatical clarification) and a German translation; each word is followed by a notation of the texts in which it is found, as well as by a handy reference to the third part of the work, in which the etymologies are treated, with listing of the most important literature. The second part contains the same words as the first (plus *obš*, p. 194), but arranged *a tergo*.

It is at once clear that this dictionary supplies a great need. Its value is naturally dependent on the care with which it is compiled, and in this respect our judgment is most favourable: it has in practice already proven to be a trustworthy guide.

It is unnecessary to give this book a recommendation: every Slavist and every comparatist who is concerned with Indo-European languages will simply be obliged to see that he has this book constantly at hand. Seldom has a new publication made its indispensability felt so quickly.

Amsterdam

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BERNHARD ROSENKRANZ, *Historische Laut- und Formenlehre des Altbulgarischen (Altkirchenslavischen)*. (Sprachwissenschaftliche Studienbücher, herausgegeben von Hans Krahe) Mouton & Co., Den Haag 1955, (in Verbindung mit Carl Winter, Heidelberg). 144 S.

One can only rejoice at the appearance of a book which has as its purpose to give a brief description of the Old Bulgarian grammar, and at the same time to take into the consideration the most important problems of Slavic scholarship. (Vorwort, p. 5).

But while reading it, our pleasure gradually became less. The work is so carelessly written (and here and there, undoubtedly, badly corrected), that one would be disinclined to put it into the hands of a beginner in Slavic studies. When one reads in the Foreword (p. 5): "Phonologische Gedankengänge konnten berücksichtigt werden; auf

die Wiedergabe phonologischer Schemata wurde jedoch verzichtet", one turns of necessity to the chapter on softening and palatalisation, which is, one knows, the chief point on which the functional and the anti-functional views of Old Slavic differ from each other. It is at once clear that Rosenkranz (pp. 55-56) accepts an "urslavisches Konsonantensystem" with, for example, three sorts of liquids: *l* ("velar"), *l'* ("erweicht"), *lj* ("palatalisiert"). This is thus in direct contradiction to the announcement in the foreword. Against the three series of consonants as such, we have no objection: it is also the view of such a scholar as Vaillant, who speaks of "consonnes dures, molles et palatalisées" (*Grammaire comparée des langues slaves*, t. I, Paris, 1950), but even Vaillant, whom one can hardly regard as a phonologist, says (p. 45): "Mais les consonnes des deux séries [mouillée et dure] ne constituent pas des phonèmes distincts, puisque leur point d'articulation reste le même et que leur prononciation dure ou molle est liée à celle de la voyelle qui suit". It is not the theoretical point of departure which is in the last analysis most important, but the manner in which it is put into practice. In the case of Rosenkranz we find total confusion. The palatalised *lj* (naturally written consistently thus by Vaillant) can be found in Rosenkranz' text represented, without the slightest degree of regularity, by *l'*, *l* or *l'j* as well (e.g. p. 12: "*l'judiže*"; p. 48: "*stel'q*"; p. 122: "*mol'j'q*"). For the palatalised labials we find in one and the same paragraph (p. 58): *pl'* and *vl'j* ("*kupl'q*" and "*stavl'j'q*"). The unprepared reader would certainly be led to think here of an extremely subtle differentiation! A similar differentiation is made on pp. 58-59: *s* (developed by the second palatalisation from *ch*) and *ś* (from the Indo-European *s* before a palatal vowel).

In the case of the vowels, also, the opposition velar vs. palatal was evidently not too clear to the author. We cite (p. 40): "Dem Urslavischen darf man . . . folgendes Vokalsystem zuschreiben:

velare Vokale: *ā, ō* (nasal), *ū, y, o, ŭ*;
palatale Vokale: *ě, ē*, (nasal), *'ū, ĭ, e, ĭ'*.

For *'q* there is no place in such a system.

The Russian *ja* appears as *ia* (p. 18: "*Viata*") and as *'a* (p. 20: "*sv'atōj*"). Further, it is incorrect to give the Serbian examples indiscriminately in their jekavian, ikavian and ekavian forms without telling which one is using: p. 18: "*svijēća, svića*"; p. 19: "*cvijet*"; "*zvezda*"; "*mlijeko*". In general, the *jat'* in the various Slavic languages

has not been very well served by Rosenkranz: p. 19: "poln. čech. *mleko*". For Russian the old and new orthographies are transliterated indiscriminately: p. 18: "*svěčá*"; p. 19: "*cvet*", "*zvezdá*". For "russ. ukr." are given on p. 19: "*gólos*"; "*górod*", and on p. 22 it appears that this example must also serve to show the development of the Old Slavic *g*.

On the subject of the morphology we shall be brief. We shall observe only the treatment of the verb *byti*. P. 123: "Die gesamte Formenreihe des Imperativs liegt vor beim Verbum *byti* 'sein'". This is incorrect; the form *boděmŕi*, which is given for the 1st p. sing. does not appear in the texts, and should therefore have been marked by an asterisk. On p. 132 this same form is given as *boděmŕi* (probably because of the only form to be found in this person: *otŕipaděmŕi*). In the case of the imperfect (p. 124) *bě* is put on the same footing as *běaše*. This, too, is incorrect, even if one rejects the view that *bě* is an aorist of the imperfective aspect, which is certainly more in accordance to the form.

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S. WESTFAL, *A Study in Polish Morphology. The Genitive Singular Masculine*. (Slavistic Printings and Reprintings, edited by C. H. van Schooneveld, VIII) Den Haag, Mouton & Co., 1956. XX + 400 pp.

In modern Polish, masculine nouns which in the nominative end in a consonant, take in the genitive singular *-u* or *-a*. Historically, the former ending is that of the Indo-European *u*-stems, the latter, that of the *o*-stems, but "in modern literary Polish ... the distribution of the two endings ... is a complex subject", as we read in the very first line of this book; so complex, indeed, that the author finds it necessary to comfort us a bit (p. XIX): "Non-Polish readers of this book may feel baffled by the mere fact, that a monograph of this size was necessary to give a more precise idea of the distribution of the endings of one single case of the masculine declension. They might feel tempted to think what a difficult language Polish is. In anticipation let us remind the reader of what has been said at the beginning of this introduction, that this particular case is the most difficult problem in the structure of modern Polish, that even distinguished Polish philologists do not escape mistakes in this 'colour-changing'

case, that this work includes far more substantives than are ever used by any single speaker of Polish, and that, finally, the work attempts a solution of the whole problem and deals with all its aspects which are of interest to the philologist".

The title of the book instructs us, that we have to do with a morphological problem. The stem of the substantive determines the choice between the two endings. The division is in principle conventional: each stem has its set ending, which only in very rare cases appears to go back to the old opposition in Common Slavic (cf. the chapter "Traditional Relics", pp. 351-352), such as the genitive *wółu* 'ox', about which Westfal says (p. 330): "*Wół* is an ancient *-u* stem ... Without this past neither *wół* nor [two other words] could ever have taken an *-u*".

The number of masculine substantives which in Proto-Slavic took an *-u* in the gen. sing. included, according to Kryński, 13, only monosyllabic stems (p. 350). All others took the ending *-a*, therefore, the great majority. At present in Polish, it is just the words in *-u* which are more numerous than those with *-a*: of the 7905 non-diminutive substantives treated by Westfal, 5062 exhibit the *-u*, 2056 the *-a*, and 787 both endings together (p. 358).

But in addition to the reversal in numerical relationship, another, more radical change can be observed: while there were in Proto-Slavic a number of isolated instances of masculine substantives which were declined in a peculiar manner (instances which the foreigner wishing to learn the language was obliged to remember individually): it is possible in Polish to distinguish a number of structural, formal and semantic categories (the terms are Westfal's), which exhibit a preference for the ending *-a* or *-u*. This categoric fixity was well-known, it is expressed in such a statement as that of Szober (quoted on p. 352), that the genitives *daru*, *domu*, *dołu* and *wierzchu* "do not correspond to present-day grammatical intuition". In other words: *dar* etc. belong to a category which as a rule takes *-a*. Such categories did not exist in Proto-Slavic.

Westfal has investigated, in the light of an overwhelming quantity of factual material, in how far such "grammatical intuition" actually plays a role. We should have liked to see his answer to this question introduced by the following considerations. When the present generation of Polish children gives the word *syn* 'son' the genitive *syna*,

they do it because they hear this form from the older generation, and certainly not because a son is an animate being and the category of animate beings always takes *-a*. If we ask ourselves why an earlier generation began to say *syna* in place of the older form *synu*, we can explain this very well by "grammatical intuition", which had established a relationship between the animate character of innumerable words and their genitive ending *-a*. In the case of the word *wół* on the other hand, the individual fixity remained supreme, so that this word is now an exception in Polish. Even in this case, Westfal gives, albeit with reservations, a semantic factor which might possibly have exerted some influence (p. 330): „the ox, being a castrated bull, loses thereby something of its animate meaning”.

It is clear that with this last pronouncement, Westfal is moving on the terrain of historical rather than of descriptive linguistics: a purely synchronic observation would not have to go farther than to remark that the gen. of *wół* is *wółu*, and that therefore the form *wółu*, if we come across it in writing, strikes us as strange. But in other cases, the situation is otherwise (although Westfal does not make this distinction). The author having found for the non-Polish word *nairn* 'omnibus in Iraq' in one case *-u* and in another *-a*, he explains these endings as follows (p. 95): "*Nairn* was the name of the organiser of omnibus routes in Palestine and Iraq . . . – its *-a* is personal, its *-u* formally determined". If this explanation is correct (and this will seem quite likely to anyone who has read the book with its numerous corroborative examples), then two categories are exposed here (the one semantic, the other "formal"), which in the modern language are living, in the sense that they exert an influence on the form of some linguistic utterances. To sum up, we can therefore say that the great number of cases such as *syna* make quite probable the existence of a semantic category 'animate', and that the no less great number of cases like *nairna* bear out this assumption. It is just these peripheral words, in the case of which the choice of the genitive ending cannot be automatic, which render so valuable the material assembled in this book.

Westfal has collected his material out of 175 thousand pages of literature. He presents it ordered in semantic classes. Within each class he treats in turn the words which always take *-a*, those which admit of both endings, and those which occur only with *-u*. The

frequency is always reported, e.g.: "*traktor*, -a (4), -u (4), 'tractor'" or "*rower*, -a (1 – exceptional), -u (many examples – normal), 'bicycle'" (p. 94). In the case of each ending it is stated whether this is semantically, structurally or formally based.

The semantically determined -a categories are: 1. animates, 2. tri-dimensional objects ... equal in size to man or smaller than man, 3. quasi-animates, showing a marked shape and a pronounced individuality (like trees or statues), 4. mobile objects, 5. words with a depreciative value (like *gruchot* 'old dilapidated thing', p. 304). The semantically determined -u categories are: 1. objects exceeding the upper size limit, 2. material nouns, 3. bi-dimensional (e.g. geometrical surfaces) or practically bi-dimensional (flat) objects, 4. abstracts, 5. collective substantives.

It goes without saying, that an object can belong at the same time to different semantic categories, as is the case, for instance, with trees (p. 104).

On pp. 337–338 the author gives a survey of the cases of semantic determination (albeit not complete). But we look in vain for such a survey of the formal and structural categories. The author says in his Conclusion (p. 347): "It is impossible to mention here all the formal types established in this work". Here we do not agree: in a book of 400 pages, one should not omit an important part of the conclusion in order to save three pages. We regard this as a failing in the work. No less regrettable is the absence of an alphabetical index of the words discussed. Such an index would have made the book more useful as a reference work.

The difference made by Westfal between structural and formal determination does not strike us as satisfactory: it artificially brings diachrony to bear on a synchronic description. Thus, the author's criticism of Grappin (pp. 343 and 346) is unfounded, that he ignores the difference between such words as "*komentarz* and *lichtarz* [which] are not Polish formations" (thus: a "formal type") and "*wachlarz* which is of Polish formation" (thus: a "structural type"). In present-day Polish these words quite simply have the same final syllable, which can, in spite of the different historical origins, form the basis for a formal category. Moreover, Westfal himself says (p. 347): "Occasionally, the problem of whether to regard a type as structural or formal is not altogether simple".

We have already cited above the passage in which the author explains that "the work ... deals with all ... aspects which are of interest to the philologist". But what applies to the philologist does not apply to the linguist. The latter remains unsatisfied in one respect: the question whether the two endings *-u* and *-a* have a different semantic value is not answered, but everything points to the fact that the author's point of departure at the very beginning of the work was, that there was no such difference. It is well known that Roman Jakobson has attempted to demonstrate such a semantic difference for Russian, in which the same two endings have been retained from Proto-Slavic. It seems to us that we can say with certainty (Westfal does not mention this), that the context is never decisive for the choice between *-u* and *-a* (which is in fact the case in Russian). For that reason it is possible, when looking for a semantic differentiation, to use all cases in which the same word exhibits both endings in turn. It goes without saying that for this purpose one cannot regard two homonyms as "the same word". Now Westfal's analysis becomes exactly at this point somewhat unclear, since he uses the idea of "homonym" in a remarkably broad sense. For example, for Westfal, *kadtub* 'torso' (p. 23) and *kadtub* 'body of an airship, machine, musical instrument, ship, vehicle' (p. 305) are "homonyms with regard to each other". Partly because of the absence of an index, one finds the two places where the word *kadtub* is discussed only after long search or by accident. And when one has found them, it appears that Westfal's material yields the word practically always with the ending *-a*, and that in the three exceptional cases with *-u*, the meaning 'hull' is intended, and not 'trunk' (although the meaning 'hull' also appears a score of times with *-a*). We should therefore be inclined to conclude that for this substantive the ending *-a* is neutral, and aside from its meanings 'genitive' and 'singular', meaningless, whereas the *-u* is a marked ending, carrying the semantic element of large size. We should therefore like to make a difference between the *-u* which is chosen automatically (e.g. because the stem itself already implies the semantic element of large size, as in *wulkan*, *-u* 'volcano', p. 170) and the *-u* which expresses this connotation by itself (e.g. *kadtubu*). In Westfal's opinion, the *-u* in this latter case as well is conditioned by the meaning of the stem 'hull', since the meaning 'trunk' belongs to another word.

Let our opinion of the book not seem, because of these remarks,

more negative than it actually is. We must be grateful to the author for his labour, through which a problem, until now only impressionistically approached, has at last been the subject of a thorough investigation.

Amsterdam

C. L. EBELING

Tamil Culture, Vol. IV, no. 1 (January 1955), A Quarterly Review dedicated to the study of Tamilians (Madras).

This periodical is symptomatic of the need of the South-Indian sub-nations for a more balanced form of consciousness of their own character and cultural traditions. From this fourth volume onwards it bears the second sub-title "Journal of the Academy of Tamil Culture". This Academy has been founded at a meeting of scholars and lovers of Tamil held on Sept. 18, 1954. It may be anticipated that "Tamil Culture", as the organ of this newly-founded Society for the development and advancement of the Tamil language, literature, arts and sciences, will gain a still wider influence.

It would not be surprising, if the immediate result of the modern process of unification of India and the growth of a modern nation should be that in its regional components (and especially in those which have not taken a leading part in the development of India during the last centuries) the consciousness of their own character is stimulated. The different parts of India cannot be wielded together by totally ignoring their different cultural traditions. No real integration of the modern Indian nation will be attained, unless its components will have learned to accept each other as separate individuals with a different character. This integration is a gigantic task set to the next generations. It is not surprising that for the present time there should be much tension and feelings of disappointment, and the political and sociological aspects of the linguistic situation of present-day India deserve our full interest and attention. There are complaints about the position of the study of Tamil. The founder and chief editor writes "There are dark and disconcerting obstacles in the way of a dispassionate appraisal of the Tamil contribution to the culture of India and Ceylon. An American sent by the Rockefeller Foundation returned from India to America five years ago with the impression that the Government of India was not interested in any studies that would

prove the non-Aryan origin of cultural trends and historical events in India", and he warns: "The function of the research scholar is not to act as a partisan but to be a witness to the truth. It is fatal to scholarship when it is blinded by prejudice or made to serve the interests of a party". Again and again we are reminded, in this periodical as well as in the scholarly *Annals of the Madras University*, of the difficulties which our Indian colleagues experience in their attempts to keep a strict impartiality in matters which are apt to stir the national feelings. Nor can it be said, that the Editor was himself quite true to his standard when at a Tamil Festival in Singapore he "told the gathering that 75 per cent of the Indian culture and civilisation was based on Tamil" (thereby freely paraphrasing, and distorting, a casual remark of S. K. Chatterji's), after which, according to a local newspaper, "He said Tamil was the oldest language in India and the world, older than Greek, but while the latter was already dead, Tamil was living and growing and its influence was found over Asia and even the Americas" (see p. 104). Fortunately, Tamil is too fine a language, and its literature too important, to need this sort of praise.

The issue under review contains among the contributions that are of interest to the readers of this journal a very competent article on "The Present State of Dravidian Philology" by the well-known Dravidianist of the University of Prague, Dr. Kamil Zvelebil, who sums up in five pages the characteristic traits of the recent revival of Dravidian studies in Europe and abroad. There is an admirable and well-balanced account by Francis Moraes of the life and work of the great scholar Dr. U. V. Swaminatha Aiyar (Cāminātaiyar), to whose life-long activity we owe it that most of the long-forgotten works of the oldest Tamil literature have been brought to light and the memory of the brilliant Old Tamil civilisation has been restored. The article on "Transliteration of Tamil in Roman Characters & Vice Versa" by a "Senthamilan" (student of classical Tamil), though interesting in details, would seem open to criticisms. For students of Tamil the periodical contains much of interest.

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F. B. J. KUIPER

GERTRUD PÄTSCH, *Grundfragen der Sprachtheorie*. VEB Max Niemeyer Verlag, Halle (Saale) 1955. VIII, 182 S., gr. 8°, geb. DM 6,15.

Diese beachtliche Neuerscheinung zur sprachtheoretischen Diskussion zeigt allenthalben in den Voraussetzungen, in der zitierten grundlegenden Literatur und in verschiedentlich eingeflochtenen weltanschaulich-politischen Äußerungen und Folgerungen, dass die Verfasserin vom Standpunkt des dialektischen Materialismus an ihr Thema herantritt. In diesem Rahmen kann freilich nur der wissenschaftliche Gehalt des Werkes eine prüfende Würdigung erfahren.

Von ihrer eingenommenen Position aus hat die Verfasserin einen kritischen Blick für alle Schwächen der Autoren, die sich, von anderen philosophischen Richtungen her kommend, um eine Grundlegung der Sprachtheorie bemüht haben, so vor allem W. v. Humboldt, H. Steinthal, H. Paul, W. Wundt, E. Cassirer und K. Bühler. Bei der Kritik des Organonmodells, das der Letztgenannte in seiner „Sprachtheorie“ zur Verdeutlichung seiner Ansichten über das gegenseitige Verhältnis der drei Funktionen der Sprache geschaffen hat, wird m. E. Analyse mit Isolierung verwechselt. Er habe, so heisst es S. 34, die drei Funktionen Ausdruck, Appell und Darstellung gewaltsam auseinandergerissen. Bühler sieht sie aber dennoch in ihrer naturgemäßen Verflechtung, sonst könnte er (a.a.O., S. 32) nicht sagen: „Denn ein Rest von Ausdruck steckt auch in den Kreidestrichen noch, die ein Logiker oder Mathematiker an die Wandtafel malt“. Dass aber die Darstellungsfunktion gewissermaßen eine höhere Ebene darstellt, die die Ausdrucksmittel der Tierwelt nicht erreichen, diese Tatsache dient auch der Verfasserin (S. 74 u.ö.) zur Untermauerung ihrer Ansicht von der Entstehung der Sprache durch die Erfordernisse des Gedankenaustausches bei gemeinschaftlicher Arbeit. Wie Appell und Darstellung in einer einzigen Äußerung vereint sein können, zeigt auch das von Bühler verwendete 'Regenschirmbeispiel' (Bühler S. 46, Pätsch S. 58 f.), dem ich jedoch noch eine andere Deutung abgewinnen möchte. Es ist nicht erforderlich, dass der Professor den Ruf „*Es regnet!*“ jedesmal zu Ende denkt: *also muss ich den Regenschirm nehmen*. Vielmehr ist dies in Form eines Appells tatsächlich schon in der Tonmodulation, die die Äußerung durch die besorgte Gattin erhält, ausgedrückt. Ja, man kann sagen, dass hier die Darstellungsabsicht so sehr in den Hintergrund gedrängt wird, dass bei diesem Ausruf eine ursprünglich

nachfolgende Aufforderung, den Regenschirm mitzunehmen, in Form eines Zweitsinns (im Sinne G. Kandlers) dominant geworden ist. Das beweist jedoch wieder nichts gegen die Möglichkeit einer heuristischen Trennung der drei Funktionen am Organonmodell. Eine Äußerung wie „*ich freue mich*“, „*ich bin glücklich*“ kann natürlich Ausdruck sein, muss es aber nicht. Der Gehalt an eigenem Erleben, das nach Entladung drängt, ist doch unter Umständen gleich Null in einem Geschäftsbrief, worin es heisst: „*Es ist mir eine Freude, Ihnen mitteilen zu können . . .*“.

In der unbedingten Absage an die strukturelle Sprachbetrachtung (S. 45, 47) geht die Verfasserin weiter als ihre sowjetischen Fachkollegen, vgl. etwa V. A. Zvegincev, *Voprosy jazykoznanija* 1957, 2, 152 f., der die Gefahr der einseitigen Festlegung auf die historisch-komparative Methode unter Vernachlässigung der strukturellen Methode wohl erkannt hat. Ebendort (S. 153) ist mit Recht Stalins Ansicht, es gäbe keine vom sprachlichen Material unabhängigen Gedanken, als Irrtum hingestellt worden. Wie bekannt, erhoben sich ja noch im Zuge der Diskussion des Jahres 1950 aus den Reihen der sowjetischen Forscher Stimmen gegen diese Ansicht, die Stalin zu einer Entgegnung veranlassten (Tovariščam D. Belkinu i S. Fureru, vom 22. Juli 1950). Mit Stalin ist aber nun auch unsere Verfasserin der Ansicht: „... kein Gedanke realisiert sich ohne die Lautsprache, die auf einer höheren Kulturstufe in ihrer Wirksamkeit durch das geschriebene Wort erweitert wird“. Die gleiche Annahme, dass nämlich ohne eine ausgebildete Sprache kein vernünftiges Denken möglich sei, diente schon 1756 dem protestantischen Theologen und Soziologen Johann Peter Süßmilch als Argument in seinem „Versuch eines Beweises, dass die erste Sprache ihren Ursprung nicht vom Menschen, sondern allein vom Schöpfer erhalten habe“. (Vgl. seine Vorrede zur Neubearbeitung, Berlin 1766). Man muss wohl das Phänomen ‘Denken’ aus einer weiteren Sicht heraus zu begreifen suchen. Das ‘materielle’ oder physiologische Substrat jedes Denkvorgangs ist nicht nur das Wort der Lautsprache oder sein erlebtes Erinnerungsbild, sondern jeder neuroelektrische Vorgang, der zur Bildung von Engrammen führt, also auch aktuelle oder wiedererlebte Reize der anderen Sinnesorgane, zumeist wohl in ihrem ‘Gestalten’ widerspiegelnden Zusammenwirken.

Das in der Aufklärungszeit (P. L. Moreau de Maupertuis 1754) und dann wieder bei W. Wundt und N. J. Marr zu neuem Leben erwachte

alte Märlein Vitruvs von der Priorität der Gebärdensprache wird S. 27 ff. mit Recht abgelehnt, ebenso wie S. 73 ff. die evolutionistischen Anschauungen der Sprachentstehung (Interjektionstheorie, Nachahmungstheorie). Sehr richtig fragt die Verfasserin S. 76: „Woher kommt es, dass unwillkürliche Äußerungen mit einem Male einen verstehbaren, also gedanklichen Inhalt haben? Und woher kommt das plötzliche, neuartige Verstandenwerden? Innerhalb des tierischen Bereiches gibt es kein Verstehen, das eines Tages in Sprache umschlagen könnte...“. In diesem Zusammenhang steht auch ein wichtiger Beitrag der Verfasserin zur Diskussion um das Verhältnis Tiersprache: Menschengprache, indem sie sich die Forschungen des Physiologen I. P. Pawlow zunutze macht, insbesondere seine Lehre vom ersten und zweiten Signalsystem. Sie gelangt damit zu der Erkenntnis, dass dem tierischen Schreckruf die Appellfunktion mangelt. Das Schnauben des Leittiers löst in der Herde einen bedingten Reflex aus, nicht anders als der knackende Zweig unter der Pranke des herannahenden Raubtiers. Der hervorgebrachte Laut wird also keineswegs von der Herde verstanden. Im gleichen Sinne müssen aber auch die Untersuchungen zur 'Bienensprache' (Karl von Frisch, *Bees, their Vision, Chemical Senses and Language*, Ithaca 1950) interpretiert werden. Neben der Mitteilungsabsicht fehlt der Tiersprache ebenso wie der tierischen Arbeit aber noch die freie Variabilität, wie das die Verfasserin auch erkennt (S. 67, 71, 76). Nur der Mensch kann deshalb lügen. Es ist also richtig, wenn S. 67 festgestellt wird, dass 1. die Arbeit, 2. das Denken und 3. die Sprache den Menschen vom Tier unterscheidet, nur muss man dann die tierhaften Vorstufen dieser drei Merkmale anders benennen. Auch S. 68 ist der Satz: „Und als Folge der Arbeit entstand mit dem Denken die Sprache“ nur unter dieser Bedingung verständlich. Ist hier aber nicht vielmehr Ursache und Anlass verwechselt? Vgl. S. 73. Das dialektische Verhältnis kommt – ohne Wiederaufnahme des Ursprungsproblems – S. 143 f. viel besser zum Ausdruck: „Die Entwicklung des Denkens und die Entwicklung der Sprache stehen in Wechselwirkung, das eine fördert das andere. Ihre unmittelbare Verbundenheit stellt sich nicht einfach als sprachliche Formengebung gedanklicher Inhalte dar, sondern sie ist weit komplizierter. Das Denken hat die Wirklichkeit zum Gegenstand, aber die gedankenmäßige Verarbeitung des Aufgenommenen ist als Prozess in seinem ganzen Verlauf an die Sprache gebunden. Die Sprache ist

ihrem Signalcharakter gemäß kein in Laute umgesetztes Abbild des Denkgegenstandes. Sie ist ein Materielles, das als Zeichen dem Bezeichneten notwendigerweise erst mit dem Denken zugeordnet wird. Dem Denken steht die Sprache in dieser Rolle zugleich aktiv und passiv gegenüber. Sie ist aktiv an der Entstehung der Gedanken beteiligt, ohne sie ist ein Umsetzen der Erfahrung in gedankliche Erkenntnis unmöglich (letzteres mir fraglich, J. K.). Sie ist aber zugleich vom Denken geprägt, ohne Abstraktion ist wiederum die Entstehung der grammatischen Kategorien unmöglich. Sie ist also Voraussetzung und Ergebnis zugleich". Im gleichen Verhältnis zueinander stehen aber auch nach S. 55 Sprache und Gesellschaft, oder S. 73 die menschliche Arbeit einerseits und Sprache und Denken andererseits. Zur tieferen Erkenntnis dieses Verhältnisses fehlen dem Rezensenten nach einer in den *Voprosy jazykoznanija* Jg. 1953, 3,22 f. geäußerten Ansicht die Voraussetzungen. Warum haftet aber die Verfasserin S. 88 u.ö. am Wort von der Widerspiegelung der Wirklichkeit im Denken, während doch ihre Ausführungen ebensooft beweisen, dass der Denkprozess viel eher einer Bewältigung und Beherrschung der Umwelt gleichkommt, natürlich in dialektischer Einheit mit der gesellschaftlichen Produktionstätigkeit?

Allenthalben gibt es Gelegenheit, die Verfasserin um ihre Kenntnis der außereuropäischen Idiome zu beneiden, die ihr die Möglichkeit geben, die Kinderschuhe lateinisch-griechisch orientierter Sprachbetrachtung abzustreifen, die ja leider die früheren Sprachtheoretiker so vielfach gehemmt haben. Man würde aber S. 92 doch mehr Rücksicht auf die zünftige etymologische Forschung erwarten: die vorgebrachte Etymologie von *mensa* ist unhaltbar. S. 162 f. ist nicht einzusehen, warum die unterschiedliche Behandlung des georg. Auslauts bei der Entlehnung ins Batsische durch die grammatische Funktion des angefügten *-i* bedingt sein sollte, es ist doch gerade das „lautliche Verhalten“ aller auslautenden Vorderzungenvokale (einschließlich *-a*) und das numerische Übergewicht der Kasus, die mit Wegfall dieser Laute gebildet werden, dafür in Rechnung zu stellen. Bestünde die Ansicht der Verfasserin zu Recht, dann könnte es im Deutschen nicht *Koks*, *Keks*, oder *Rips* heißen.

Die ganze durch Stalins Eingreifen in die Sprachwissenschaft ausgelöste Diskussion über die Existenz von „Klassensprachen“ zeigt eigentlich nur die Mehrdeutigkeit des Terminus „Sprache“. Statt hier die

Grundbegriffe zu klären, wozu Stalin als Nichtfachmann natürlich nicht imstande war, sitzt die Verfasserin S. 118 ff. über W. Porzig zu Gericht, nur um zu der schon in der Überschrift festgehaltenen vorgefassten Meinung, es gebe keine Klassensprachen, zu kommen. Hätte nicht Marrs Pseudowissenschaft hier Unheil gestiftet, man wäre wohl kaum darauf verfallen, die klassenbedingten Unterschiede im *Sprachgebrauch* als verschiedene Sprachen aufzufassen. Vgl. etwa G. Ipsen, der 1930 (Bl. f. dt. Philosophie, 4. Bd., S. 8) schreibt: „Wie Sprache im Raume nach Stämmen, Ländern, Staaten, Städten verschieden ist, so unterscheidet sie sich auch gemäß dem Aufbau der Gesellschaft und nach der Gliederung des Volkes in Berufe. Offenbar aber bedeutet diese Unterscheidung in Gruppen- (oder Fach-) sprachen keine echte Besonderung der Sprache, denn das Gesetz ihrer Bildung und der Grund ihrer Unterschiede liegt nicht sowohl in der Sprache, sondern in der bestimmten Gruppe und ihrer eigentümlichen Welt“.

Beachtlich sind nicht zuletzt die Erörterungen über den Vorgang bei der sprachlichen Abstraktion, woraus sich eine Auseinandersetzung mit E. Albrecht, Greifswald, ergibt (S. 138 ff.). Wenn urtümlichere Sprachen im Wortschatz Unterscheidungen treffen, die den Kultursprachen abgehen, so dürfe daraus nicht auf den Mangel einer logisch-systematisierenden Abstraktionsfähigkeit geschlossen werden. Vielmehr handelt es sich jeweils um das Interesse einer primitiven Produktionsgemeinschaft an den für die Produktion wichtigen Objekten der naturalen Umwelt. Die Gegenprobe für die Richtigkeit dieser Ansicht ist es, wenn vielfach für unser Denken Heterogenes in den Natursprachen in generellen Bezeichnungen subsumiert wird, was man leicht an Farbbezeichnungen aber auch sonst feststellen kann.

Verschiedentlich ist es von Nachteil, wenn die Erklärung für eine Erscheinung nicht beim Menschen, als dem Sprecher, sondern beim sprachlichen Material gesucht wird, ein Fehler, dem auch der Strukturalismus verfallen ist. Die heute wieder deutlich erkennbare Verschiedenheit der Meinungen in den einzelnen linguistischen Schulen zeigt, dass das Phänomen Sprache durch seine Vielschichtigkeit geradezu dazu verlockt, es unterschiedlich zu beurteilen, indem es sich einer Annäherung von den verschiedensten Standpunkten aus darbietet. Der Forscher, der für seine Lösung die Ausschließlichkeit beanspruchen wollte, würde unweigerlich in die Irre gehen.

THEODORO HENRIQUE MAURER Jr, *Dois problemas da Língua Portuguesa: O Infinito Pessoal e o Pronome SE*. (Universidade de São Paulo, Faculdade de Filosofia, Ciências e Letras, Boletim 128, Filologia românica No. 3.) São Paulo 1951, 71 pages.

Le travail que présente M.M., le savant professeur de langues romanes à l'université de São Paulo, réunit deux études sur des problèmes intéressant particulièrement la langue portugaise, à savoir l'infinitif à désinences personnelles, dit l'infinitif „personnel” ou „fléchi” et le pronom *se*.

Quant à l'infinitif personnel, cette innovation morphologique, la plus originale du portugais, l'auteur discute les principales théories sur l'origine de sa flexion, tout en critiquant la théorie de l'imparfait du subjonctif, actuellement assez en vogue. Il considère ensuite la théorie de Otto, Leite de Vasconcelos et Coelho, pour qui cette forme spécifique proviendrait directement de l'infinitif commun des langues néolatines par la transférence des désinences des „modi finiti” à des constructions personnelles, c.à.d. pourvues de sujet, de l'infinitif. C'est à cette dernière hypothèse qu'adhère M.M., en la basant sur une nouvelle explication.

Il nous semble que l'auteur a bien réussi à nous démontrer 1° l'insuffisance de l'hypothèse généralement acceptée de la dérivation de l'imparfait du subjonctif latin et 2° la justesse de la vieille théorie de la provenance de l'infinitif roman, théorie qu'il défend et expose de nouveau, en insistant sur le fait que l'infinitif peut admettre un sujet nominatif en Espagne et sporadiquement aussi dans la Romania médiévale entière, ce qui nous paraît exact, mais ne corrobore pas sensiblement la thèse soutenue par l'illustre linguiste brésilien.

Dans sa deuxième étude l'auteur examine l'emploi du pronom réfléchi *se* dans la formation de la voix passive et des expressions impersonnelles qui désignent un agent indéterminé. Dans une phrase comme *come-se bem neste restaurante* M.M. regarde *se* comme une expression impersonnelle développée à partir d'une expression passive. Selon lui *se* dans une telle phrase serait un morphème qui, uni au verbe, lui donne un sens impersonnel, en se rapportant à un agent indéterminé. M.M. appelle ce morphème une „particule d'impersonnalisation”. Ensuite il traite longuement de la formation et de l'évo-

lution sémantique de la voix médio-passive dans les langues indo-européennes et il arrive à la conclusion suivante: „L'étude de la voix médio-passive dans les langues indo-européennes et l'examen de la forme analytique de la conjugaison réfléchie dans les langues romanes révèle le même procès évolutif: une forme à valeur moyenne ou réfléchie sert à exprimer une action qui se concentre dans le sujet ou sur le sujet et, surtout avec l'omission de celui-ci, une action générale, réalisée par un agent personnel indéterminé. Tout en prenant ce nouveau sens, la forme médio-passive perd la notion primitive de passivité, parfois à un tel point qu'elle finit par admettre un complément direct, chose entièrement inconcevable dans son sens littéral et primitif."

Malgré la similitude de leur évolution sémantique, nous ne sommes pas convaincu du rapport organique que l'auteur veut établir entre ces deux types syntaxiques de même nature qui se sont produits à des époques différentes et dans des régions sans contiguïté.

Leyde

H. L. A. VAN WIJK

Novum glossarium mediae latinitatis ab anno DCCC usque ad annum MCC edendum curavit Consilium Academiæ Con-sociatarum; huic fasciculo [L] conficiendo præfuit Franz Blatt, universitatis Arhusiensis professor. Hafniæ, Ejnar Munksgaard, MCMLVII. VIII pp. and 232 col.

Index scriptorum mediae latinitatis ab anno DCCC usque ad annum MCC qui afferuntur in novo glossario ab academiis conso-ciatis iuris publici facto Hafniæ, Ejnar Munksgaard, MCMVII. 194 pages.

Tempora mutantur ... In 1678 the lawyer Charles du Fresne, sieur Du Cange, published his *Glossarium ad scriptores mediae et infimae latinitatis*, followed in 1688 by a *Glossarium mediae et infimae graecitatis*, and in 1680 by his edition of the Byzantine historians. It is told, that he worked every day of his life, with the exception of his wedding-day, and that he never forgave himself this interruption.

Three centuries later, we witness the slow and difficult birth of a new comprehensive Glossarium, product of the collaboration of the united academies and of the zeal of many hundreds of scholars, and patronized by the Unesco. But in the preface of this first fascicle L the managing editor Franz Blatt warns us that the new glossary does

not pretend to replace the old Du Change. Firstly, the present work only covers the latinity of the 9th–12th centuries. Secondly, the historical and technical explanations have been reduced to a minimum. This means that the material from the 2nd century (*Thesaurus Linguae Latinae*) up to the 8th century will have to wait for another special glossary, and the same applies to the 13th and following centuries. The other limitation implies that the substantial body of 19th and 20th century studies on medieval history and institutions is ignored. In my opinion this distinction between lexicography and the historical substratum is based on the fallacy, that the meaning of a word can be detached from the thing it designates.

The historian's and the lexicographer's objections to this Du Cange *dimidiatus* have been formulated by professor Niermeyer in an extensive review, to which I refer my readers (*Le Moyen Age* 1957 p. 329–360). To his serious criticism he is the more entitled, as he is himself the author of a medieval lexicon. His lexicon has all the advantages of a one-man's work: accuracy, homogeneous elaboration. But it also has the inevitable disadvantage of a limited word-material. So after all we may perhaps be grateful for the much-disputed coincidence of Niermeyer's Lexicon and the new Glossarium. They supplement each other, and, in a way, make up for each other's defects.

But in respect to the sieur Du Cange we remain what we were: *nani in umeris gigantis*.

University of Amsterdam

A. D. LEEMAN

FRITZ KRÜGER, *Problemas etimológicos; las raíces car-, carr-, y corr- en los dialectos peninsulares*. Biblioteca de Dialectología y tradiciones populares, IX, Madrid, Consejo Superior de Investigaciones Científicas, Centro de Estudios de Etnología Peninsular, 1956, 168 páginas, 4°.

Tireless, now as in his youth Prof. K., who has written so many thorough studies of "Wörter und Sachen" of the Iberian peninsula, especially of the Pyrenees region, has recently published another detailed study of a great number of terms from various Spanish dialects. The terms he discusses are those beginning with car-, carr- and corr-. It is evident from the text as well as from the footnotes that Prof. K. not only is widely read but has an admirable knowledge of country life and of folklore.

Santpoort

J. A. VAN PRAAG

MARTINO MARIO MORENO, *Il Somalo della Somalia. Grammatica e Testi*, ed. Istituto Poligrafico dello Stato (Roma, 1955). A Cura dell' Amministrazione Fiduciaria Italiana della Somalia: pp. VIII + 404.

Prof. Moreno, the great Italian expert on the Cushitic languages of North-eastern Africa, especially on Galla ¹⁾, now has enriched the abundant literature on Somali linguistics by a conscientious description of three of the most interesting dialects of Somali, up to the standards of modern linguistic methods of research. After the comprehensive studies of e.g. Cerulli ²⁾ and many other authors who made Somali the object of their special interest ³⁾, the author has concentrated his investigations on the dialects of Benâdir, Dârôd and Dighil, each dialect being described morphologically, syntactically and, finally being illustrated by an extensive selection of annotated texts. The Author is to be congratulated on the manner in which he has solved the (in this case) difficult problem of distinguishing between "phonetics" and "phonemics" which always constitutes a moot point where the so-called "primitive" languages of Africa are concerned.

Oegsteest

H. P. BLOK

¹⁾ Among his numerous publications on the subject we may refer i.a. to his *Il ciclo di Rās Tafari nella Poesia Galla* (Riv. d. Stud. Or., Roma XV. 1934, pp. 143 seq.), *Alcuni racconti Galla* (ibid., XVI. 1935, pp. 98 seq.), *L'accento in Galla, osservazioni e orientamenti* (ibid. XVI. 1936, pp. 184 seq.), *Le mie indagini linguistiche nel Galla-Sidama* (Oriente Moderno, 1938), *Introduzione alla lingua Ometo* (Milano, 1938), *Grammatica della lingua Galla* (Milano, 1939), *Favole e rime Galla* (Roma, 1935).

²⁾ Cf. E. Cerulli, *Somalia. Scritti vari editi ed inediti* (Roma, 1957).

³⁾ I.a.: L. Reinisch, *Die Somali-Sprache* (III Th. Süd-Arab. Exped., Wien 1900, 1902, 1903) which still remains to be the standard work of reference; J. W. C. Kirk, *A Grammar of the Somali Language* (Cambr. Univ. Press, 1905); R. C. Abraham, *Principles of Somali* (1951); C. R. V. Bell, *The Somali Language* (1954); cf. H. A. Wieschhoff, *Anthropological Bibliography of Negro Africa* (Am. Or. Series XXIII, New Haven Conn. 1948), pp. 362 seq.; M. von Tilling, *Somali-Texte und Untersuchungen zur Somali-Lautlehre* (Beih. z. Zeitschr. f. Eingeb. Spr. VIII. Berlin, 1925) and the many contributions to the subject in general by Prof. A. Klingenheben and other scholars. Cf. in general M. A. Bryan, *The Distribution of the Semitic and Cushitic languages of Africa* (ed. Int. Afr. Inst., London. 1948), pp. 20 seq.

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